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The Role of Research in the Unification of a Discipline

WARREN E. MILLER
University of Michigan

Presidential Address, American Political Science Association, 1980

Large-scale complex research project designs are providing a new impetus to the elimination of subfield boundaries within political science. Major projects are taking advantage of the methodology and technology of contemporary social research to include comparisons among institutions, across cultural boundaries, and extending through time. As a consequence, traditionally narrow field and subfield concentrations on segments of the political process are giving way to intellectual interests that bring together hitherto separate concerns. The full potential for discipline-unifying research will, however, not be realized until there is a strengthening of the organizational infrastructures for research, a broadening of training in research design and administration, and an increase in funding for large-scale projects. The execution and subsequent intellectual exploitation of large research projects will carry additional problems that will be solved only with substantial changes in the workways of the political scientist, but those problems are greatly outweighed by the positive contribution that such research will make to the future of the discipline.

The growing capacity of American scholars to study politics and governance systematically rather than segmentally has brought us to the beginning of a new era. We should contemplate the future, understanding that political science can and should become a unified discipline. Methodological differences no longer define nor are defined by substantive interests. Many of the boundaries between traditional subfields are sustained only by the inertia of custom; the boundaries can and should disappear. The intellectual challenges of the discipline are increasingly, and appropriately, centered on complex interrelationships that are being explored in major research endeavors. And, because inexorable trends in all of the social sciences, as well as in national science policy, are fostering large-scale research that purposively brings many interrelated interests together in single projects, I believe political science should prepare for a new era in which large-scale, programmatic research activity will give a new and more coherent shape to the discipline.

The historically separate origins of the various subfields of the discipline are now less relevant than the intersections that today connect the subfields. Drawing from my own field of electoral behavior, one can ask how, in a day of presumed single-issue politics, the study of electoral behavior can be intrinsically separated from the study of bureaucratic clienteles that help shape the public policies that are reviewed by citizens engaged in retrospective, issue-oriented voting? How can judicially mandated educational policy be understood or appreciated with no regard for the politically potent demands of affected voters? How can the ebb and flow of foreign policy be comprehended without a basic recognition of the bounds by which public outrage defines the permissible? How can the performance of an office of personnel management be appraised without regard for the electorally inspired concern for civil rights?

More generally, it seems to me there are two logical imperatives responsible for the movement away from the specialization and compartmentalization of intellectual activities that has characterized political science. One of the continuing impulses toward unification of the discipline comes from the very nature of our subject matter. Political science is unique among the social science disciplines in its preoccupation with the institutions of politics and government, their interaction, and the relationships between the institutions and the citizenry. The very concepts of political system, political process or governmental structure properly imply aggregations of activities that are by their nature interconnected and interdependent. The separateness of subfields which have focused on various dimensions and components of the political process has existed despite, and not because of, the interconnectedness of the phenomena around which subfields have been organized.

A second impetus toward disciplinary unification comes more recently from the appreciation and implementation of the basic logic of social inquiry. The development of new approaches to social research, relevant to many problems of political analysis, has diminished subfield parochialism in research and promoted research that is responsive to the interconnectedness of the phenomena we study. In the absence of a practicable mode of carrying out research on large-scale, complex institutions and widely dispersed, mass populations in laboratory settings, the systematic analysis of political phenomena has self-consciously turned to two different modes for testing those parts of political theory that have an empirical as well as a normative referent. The more familiar mode is comparative, searching across more or less similar institutions for insight into that which makes them different despite their similarities.

The second mode, with an equally long tradition, is that of diachronic analysis in which the passage of time (or the accumulation of history, if properly documented) produces change in individual institutions and in the interaction among institutions. The understanding of such change through time complements synchronic analysis of institutions across cultural space. Both modes direct attention to the context provided by functional interrelationships and thereby force a broadening of perspective and analytic grasp; description of the discrete, bounded institution ultimately must give way to an expanded scope of inquiry.

The increasing unification of our discipline as a scientific enterprise has been made possible largely because of the development of new methods of social research. In particular, the post-World War II growth of methods and techniques, often associated with the behavioral mode, has been vitally important, less because of the intellectual ferment surrounding questions of epistemology and more because those methods and techniques have increased our powers of observation. The techniques of contemporary social research have expanded the scope and refined the accuracy of our observations of the real worlds of government and politics. They have made it possible to create evidence that is needed to establish empirical regularities and to test the explanatory theories that transform those regularities from intellectual trivia into matters of important intellectual concern.

In the absence of our generation's research methods, the older, traditional empirical components of political science were derived almost entirely from "process produced" information. Apart from personal memoirs and collections of anecdotes, the evidence on which the discipline's traditional research rested was created for the purposes of the participants in the political process. The study of comparative politics, for example, was largely limited to the textual examination of constitutions, legal codes, tables of administrative organization, financial reports of budgets and expenditures, and tabulations of aggregated election returns. These "hard data" were not developed to serve the purposes of the research scholar, but to facilitate the functioning of this or that institution. Even the discursive textual materials, the treatises that articulate political philosophy or the narratives that describe political experience, were usually produced by self-interested participants whose rhetoric was intended to serve personal or political, value-laden ends rather than the needs of the scholar.

With the development of the new modes of social research, the scientist-scholar has been freed from a dependence on information that is created for the participants in the political process. We can now elicit additional information and thereby create relevant evidence for the relatively precise and very specific needs of systematic inquiry. The development of sampling theory and the methods of applying it to very large human populations, including populations of organizations, make possible an efficiency in data collection that increases the researcher's observational power by many magnitudes. The structured personal interview and the questionnaire provide vehicles for highly standardized and highly comparable descriptions of personal experiences, perceptions, attitudes and behaviors. These new methods and techniques of eliciting information, driven by hunches, curiosities, perceived anomalies of explanation, or, formal theory, and controlled by increasingly sophisticated elaborations of classical experimental design, make it possible to create new knowledge at a rate that challenges our capacities to think new thoughts and achieve new levels of understanding.

It is true that the initial application of the new methods proceeded very unevenly. Following the so-called law of "the child and the hammer," new research tools have often been used rather indiscriminately, and with too little recognition of their lack of relevance for some of the intellectual constructions that scholars have envisioned. At the same time, the invention of new tools for research has also led many of our most creative scholars into a sustained preoccupation with the assessment of existing methods and the invention of still more new methods. From one perspective, these preoccupations with methodology have been criticized as a diversion at the expense of a continuing interest in the substantive problems which should impel and justify methodological innovation. It is undeniably true that methodological domains often acquire a life of their own, and the continued growth of these activities is still very much with us. However, very often methodological developments, particularly in domains concerned with data analysis, are stimulated by the methodologists' recognition of the inadequacies of the data available for scientific use.

It should also be noted that the use of the new tools of research has often led to the posing of new substantive questions—often questions quite remote from the immediate study of political institutions. The sheer ability to study individual behavior more directly, systematically, and comprehensively has led to an emphasis on theoretical problems confronting our understanding of individual behavior. Indeed, for a time, microanalytic concerns threatened to supplant macrolevel, institutional analysis. More recently, and more happily, our abilities to investigate at both the micro and macro levels have been recognized in the design of research that is more faithful to the real world of people and institutions. This is vitally important for the ultimate unification of the discipline because it emphasizes the relevance of empirical, social science for the central questions of both the normative and empirical theories of the discipline.

The last two decades have witnessed the production of a number of prototypic studies that, taken one by one, have added to our understanding of political phenomena and, taken collectively, demonstrate our intellectual and methodological capacity to carry out research projects of substantially greater power and broader relevance than those of earlier years. These are projects that, in scope of complexity, at least start to match the scope and complexity of the real world of politics and government. Moreover, these studies demonstrate that it is possible to do scientifically rigorous work that is sufficiently complex to be commensurate with the intricate problems we are trying to understand or solve.

For example, there is now a long catalog of studies that, by design, embrace two or more different sets of political actors participating in the patterned behavior of two or more political institutions. These studies can be illustrated by Herbert McClosky's pioneering investigation of agreements and disagreements between delegates to nominating conventions and their parties' rank-and-file constituents (McClosky, Hoffman and O'Hara, 1960). The same era produced the first study of the direct linkages between members of a national legislature and their constituencies (Stokes and Miller, 1962; Miller and Stokes, 1963), and the ensuing years have seen this basic design replicated many times over. More recently in the field of electoral behavior, studies have been designed to bring together in integrated analyses the confluence of decisions of candidates and their campaign managers (Goldenberg and Traugott, 1980), political reporters who present the campaigns through the mass media (Clarke and Evans, 1980a, 1980b; Evans and Clarke, forthcoming), and the voters who are responding to the election campaigns as presented by the media (Center for Political Studies, 1978).

Because of the larger problems of logistics and culture, truly comparative synchronic studies of similar political institutions are fewer in number. Nevertheless, the study of comparative politics was clearly reshaped by the work of Gabriel Almond and Sidney Verba (1963) in their pathbreaking study of civic cultures. The fact that comparative studies need not go to the extreme of crossing national lines was demonstrated early in the 1960s by the work of Wahlke, Eulau, Buchanan, and Ferguson (1957) in their interstate study of legislative systems.

Diachronic studies that exploit the passage of time are more numerous. In latter days they are better supported than ever before because of the creation and maintenance of data bases that extend through ever increasing spans of time. Here one can hope that the emergence of new communities of scholars, such as those identified with the Social Science History Association, will add interdisciplinary richness to the task of unifying our study of politics and government.

Many studies have come into being that combine all three themes of inter-institutional, cross-cultural, and longitudinal analysis. As with a number of the earlier studies, the newer studies also foretell an important aspect of the future in their presentation of the results of the active cooperation of a number of scholars as co-investigators and coauthors for each project. The most inclusive of such studies now under way is exemplified by a cross-national, intergenerational, longitudinal study of political participation in eight Western democracies, carried out by a dozen collaborating colleagues under the general leadership of Samuel H. Barnes, M. Kent Jennings and Max Kaase (Barnes, Kaase et al., 1979).

In the same spirit, with varying emphases on the interinstitutional, cross-cultural and longitudinal themes, one can note similar work under way or completed in the Verba, Nie, and Kim international study of mass-elite linkages in local government (1971), the studies of parliaments being undertaken by the Comparative Legislative Research Center at the University of Iowa (Kim et al., forthcoming; Loewenberg et al., forthcoming), the Converse and Pierce study of representation in the French National Assembly (1971, 1979; Converse, 1975), the Cotter, Bibby, Gibson, and Huckshorn study of American state political party organizations (Cotter and Bibby, 1980; Cotter et al., 1980), or the Cain, Ferejohn, and Fiorina comparative study of the American Congress and the British House of Commons (1979a, 1979b, 1980a, 1980b).

Perhaps it is no more than coincidence, but our demonstrated ability to carry out very complex research designs that produce relatively faithful mappings of the interrelated elements of the real world is occurring at the same time that new intellectual foci promise a further integration of political scientists' concerns. Both the new emphases on policy analysis and on political economy seem to me to demand great intra-disciplinary integration. Indeed, much of the nonideological excitement in both developments comes from the new scope they bring to the work of those doing research as well as those committed to action and application. The concept of the political process as a continuing process fits very neatly the theme of studying persistent, normatively important, social or economic problems. When problem-solving policies are examined in the context of the social and the economic, as well as the political structure of a policy, the need to know more and more about more and more becomes pervasive.

Although the time seems ripe, there are a number of obstacles to overcome before political science will come of age as a relatively unified social science discipline. If disciplinary scholars are to be provided with the information and evidence that is needed for major advances in our understanding of politics and government, institutional changes within the discipline will have to occur on at least three different fronts. As things now stand, political science as a discipline cannot respond to its intellectual challenges, or take advantage of the opportunities that those challenges present, because of inadequate personnel, inappropriate institutional arrangements for meaningful research, and limited financial resources with which to carry out truly significant research.

The prime challenge presented by our new capacities and the new potential lies in the fact that their exploitation rests on individual scholars. My remaining comments center largely on problems associated with the institutionalization of complex, large-scale research, but they are pertinent only if such research is guided by the concerns of intellectually creative individuals. The significant contributions of the research that is reshaping our discipline all have their origins in single minds and the capacities of single individuals. Their unique contributions are too often lost as credit is distributed among all of those individuals and organizations required for the execution of a major research project.

The point must be made, and is here recognized, that the scope of large-scale research does not limit, but enhances the importance of the individual scholar. In each subfield the creativity of numerous individual scholars could be identified as the ultimate source of our new knowledge. And it is not the direct participation of these individuals in large-scale social research projects that is crucial; but without their guiding insights and their synthesizing wisdom, our new methodological and organizational capacities would not have remade those portions of the discipline to which their work has contributed. The impact of the research of the future will likewise depend at least as much on the ability of individual scholars to utilize and exploit our new research capacities as it will on the continued and expanded existence of the institutions which provide them.

Although political science training relevant to large-scale social inquiries is infinitely better today than it was two decades ago, it still has major deficiencies. First of all, too few political scientists are trained in the tasks of designing research and collecting data. Whether to take advantage of those new data that have been produced, or to overcome the limitations in those data, our training in empirical studies has emphasized data analysis and data manipulation rather than the generation of new data. Too few political scientists have first-hand experience in designing research that will faithfully reflect the real political world. Numerical theory, and data analysis, will always be vital, but their exploitation must be complemented by expertise in research design and in the conduct of complex, multipurpose data collections rooted in familiarity with politics in action.

There are also too few political scientists who are professionally trained for multilingual, cross-cultural research. Although there has been a certain logic to treating research methods and statistics as research tools that are equivalent to the command of a second or third language, the equivalency is relevant only in those cases where one finds within a single language or cultural area the historical record or the inter-institutional variation that makes rigorous research possible. And, of course, for many inter-institutional comparisons the other institutions are necessarily found in other national settings. I am thoroughly persuaded that the comparative method lies at the heart of our discipline and the power of the comparative method can be fully exploited only by those who command the linguistic and other cultural understandings necessary to specify the real equivalencies in institutions across cultural boundaries. The study of empirical research methods is crucial, but it is no substitute for the training needed to study institutions in cultures other than one's own.

Perhaps the greatest deficiency in the ranks of today's research scholars is the absence of the appetite and the imagination needed to launch and then carry out large-scale research. Coming out of an era in which complex research projects could be mounted only upon rare occasions, we have taken a nettle for a rose and have emphasized the exploitation of data collected by others. As all can appreciate, I am personally persuaded that there is indeed much to be said for the data archive movement (ICPSR, 1979) and fuller use of data which it has made possible. I suspect, however, that the ability to do meaningful research with data collected by others has seduced many scholars into "making do," into molding their research interests to those tasks which are more easily accomplished, and thereby forestalling the commitment that produces the extraordinary effort that is required to launch the most relevant and most needed new research efforts.

Today, unhappily, limitations on the capacity of individual political scientists are matched or exeeded by limitations in the institutional settings needed for the conduct of significant new research. A second obstacle exists in the fact that the discipline is still served almost exclusively by academic institutions that are admirably suited for the conduct of library research or the secondary analysis of archival data, but not at all equipped to support the generation of significant new evidence appropriate to the testing of important social-political theories.

Despite repeated efforts over the past two decades to establish large social science centers to be the equivalent of the astronomer's observatories or the physicist's accelerators, no new centers of first magnitude have come into being. Indeed, many smaller local institutions have withered and disappeared. Given the magnitude of our research needs, and the importance of the social, political, and economic concerns they would ultimately serve, it is hard to understand why the National Opinion Research Center at Chicago and the Institute for Social Research at Michigan continue to stand as relatively unique institutions in a nation as large and as rich as ours. Past efforts of leaders in the social science community to develop and implement policies that would see these two venerable institutions joined by others of similar capacity have failed almost completely. Both the BASS Report (National Academy of Sciences, 1969) and the Simon Report (National Research Council, 1976) which followed it recognized the fact that the institutional infrastructure for social science needed massive expansion if our claims to intellectual or social relevance were to be redeemed. Both reports have been ignored by those who shape national science policy as well as by those who control access to the financial resources that would be needed for such a change.

In like manner, there has not been an adequate response to the need for increased social science research capacities at the university level. Some collective efforts, such as represented by the Inter-University Consortium for Political and Social Research, have persisted with minimal commitment of resources by the participating universities. Occasional departments of political science, blessed with unusually energetic and tenacious leadership, have developed data laboratories. Significantly, however, these and other limited tales of success across the nation pertain almost entirely to facilities for the secondary analysis of data collected by others for other purposes. Although it is no longer extraordinary to find departmental facilities for training in data analysis, the number of departments served by anything more than the most rudimentary, ad hoc capacity for the design and conduct of new research has not increased. To my knowledge, no new major academic resource for conducting original research and generating new data has come into being within the past 10 years.

As a consequence of the limited number of sites where new research projects can be initiated, there has been virtually no training base for research careers. The post-doctoral research assignment is virtually unknown in our discipline. Because of the same poverty of institutional resources, there is no base for the training of research administrators.

The third obstacle to the realization of the promise of a unified, rigorously scientific discipline that is moved and shaped by new research lies in the ubiquitous problem of funding. Whether cause or effect, the level of funding now available for basic political science research is totally inadequate. It is not adequate to maintain a high level of secondary research effort, to say nothing of supporting a portfolio of significant new data-collecting enterprises.

When one examines university support for research, the prevailing norm for the social sciences is still appropriate, at best, for individual efforts modeled on classical library research. Apparently, it is generally considered adequate to pay for a bit of released time for a scholar which, when added to uncompensated evenings and weekends, is sufficient to permit the writing of articles or a book. A serious attempt to collect a significant body of data as new evidence can be mounted only if the researcher seeks funding elsewhere or devotes months, if not years, of individual effort to data collecting. Funding to permit the gathering of evidence which will make possible the subsequent serious intellectual endeavor is extremely limited.

When one turns to the private foundations, the generally low level of support available to political scientists is exacerbated by the maintenance of the philanthropic "do-good" tradition. Those foundations that are interested in significant political problems are most likely to support the writing of a solution-promoting book. Only on rare occasions can a private foundation be persuaded to support a research project that does not promise amelioration but, instead, has data collection and theory testing as significant components. In many instances, of course, foundation resources are limited, and a significant social science research program would quickly spend the staff out of its job. For whatever reason, the private foundations seem wedded to a policy of providing "seed money" to the social scientist, while delegating to unspecified "other" sources the responsibility for providing "harvest" money that would transform planning and thought into research and new thought.

With neither the universities nor the private foundations willing to supply funding for large-scale research, the quest for money usually turns to some agency within the federal government. Limitations on funding levels there also remain disastrously low within such sources of basic research support as the National Science Foundation. When one moves beyond NSF, the philanthropic "seed money" theme of the private foundations is replaced by the need for "cost-effective" investment in problem-solving research supported by government agencies. Mission-oriented agencies within the federal government do indeed support large quantities of research done by social scientists, even by political scientists. However, the emphasis on application is pervasive. It is seldom possible, from the researcher's perspective, to put first things first, and first conduct the appropriate basic research. The problem is doubly severe because there is such widespread pressure, greater now than before the social and political revolutions of the last decade, to demonstrate that one's work is immediately relevant to solving some part of the nation's problems.

Political research is perhaps also uniquely vulnerable when it seeks funding from government agencies that are, in turn, dependent on the political process for their very existence. In part this is true because political self-interest often dictates the response to proposals for research programs, and the worlds of the politically sensitive and the social scientist are poorly knit together in the resulting conflict of interests. We are also perhaps uniquely vulnerable in that those involved in the chain of decisions that would produce support for our research are often the very actors whom we would study. Arguments that we need more knowledge are often dismissed on the grounds that we are simply outsiders trying to find out what insiders already know through personal experience.

The reduction or elimination of the obstacles presently posed by inadequacies of personnel, institutional structure, and funding could bring political science into a new age of intellectual ferment and maturity as a discipline. The excitement of such a prospect can scarcely be underestimated, and yet the realization of such a development would bring with it new problems that are only dimly apparent at this stage of disciplinary immaturity. Many of the problems would result from the fact that, for the foreseeable future, large-scale research will remain multipurpose research. Several correlates of multipurpose research will have to be accommodated by the workways and the norms of the discipline.

In the first place, the risks are greater. Complex research poses new problems from the beginning to the end of the research process. In planning and designing a large study, in carrying out the data collection for the study, and in the analysis of the evidence amassed by the study, there will be opportunities for delay, disappointment, and disaster of a greater magnitude than are now associated with research projects of more familiar scope. Under any circumstance, those who make their living by engaging in large-scale research are aware of the high risks involved. The most exciting research tests significant theories. Since, over the years, it has seemed virtually impossible to disconfirm any important piece of social theory with a single test, it has not been possible to publish null findings. Confirmation and extension are the hallmark of "good work," and the risk of devoting substantial time and energy to produce "nonpublishable" results has dissuaded many from attempting new and more venturesome enterprises and has turned them to less threatening avenues of intellectual activity.

In carrying out a complex research design, there is often a very real possibility that the researcher will not even get to the state of data analysis. Because of our lack of experience in research administration, the sheer management problems involved in large-scale studies offer new opportunities for failure. The large and complex study is not only extremely expensive, but also whenever such a study is innovative in any important aspect of design, the researcher's ability to estimate the ultimate cost is limited. In general, the financial costs of planning with sufficient care, executing with sufficient control, and analyzing to a satisfactory level of closure are very hard to estimate without a background of experience. Individual scholars, at least those with tenure, who confront unexpected delays and expenses in the conduct of personal research can often shift personal timetables and accommodate themselves to the unexpected. A massive research project that runs out of money before the data are collected cannot always be suspended until new resources are found. When time and money are consumed on a very large scale, and where anything short of completion leaves one no further ahead than if one had not started at all, the potential cost, whether out-of-pocket cost or opportunity cost, comes very high.

Quite apart from the intellectual and logistic problems of organizing large-scale research, another set of problems results from the fact that the magnitude of multipurpose research usually demands the cooperative participation of multiple investigators. If the number of scholars responsible for the final intellectual product increases interdependence, and the unification of knowledge, it also poses a further challenge. Unexpected interruptions and delays in the participation of any one investigator have immediate and very human implications for the others. It is often difficult to sustain a steady state of cooperation among multiple investigators whose fates are joined together by their project. Moreover, familiar problems found in smaller if not lesser enterprises are exaggerated in the large project. These originate in the initial division of labor and they continue with efforts to reflect that division in an ultimate division of rewards. At the present time, multiple authorship of articles and books is seen by many as more of a curse than a blessing. Particularly where junior authors are concerned, the question of the value to be attached to a third authorship (particularly out of alphabetical sequence) is threatening at best.

Among senior figures, serious problems can develop as the division of labor may distinguish the intellectual leaders from the chief administrators. In the instance of the national election studies conducted by the Center for Political Studies, for example, it is not clear (and will probably not be clear for some years to come) whether the principal investigator should be the intellectual first-among-equals as a scientist, primarily an expert in the politics of multipurpose research that is intended to serve many intellectual constituencies, or should be primarily an administrator who, as chief executive officer, translates the legislative wishes of advisors and other principal participants into research operations that, when carried out by the research staff, remain faithful to the conceptual superstructure by which others have defined the intellectual destiny of the project.

Whatever the internal division of labor and allocation of rewards, all of those for whom large-scale research is relevant must be prepared for a third change in our workways, a change in the time expectations associated with the completion of significant research. Given proper training, and a year or two for careful thought, a more traditional scholar may well hope to complete a book in three or four years. The number of person-years needed to produce the final results of a complex research project may increase this figure by a number of magnitudes. Junior personnel, in particular, may find the time lag in publication highly disadvantageous for their own career needs. Of course, it is also true that the continued maturation of our theoretical interests may add its own force toward encouraging the publication of the journal article rather than the extended monograph or book. In any event, our disciplinary reward system based on the timely publication of books may have to change, as it has already changed for economics and psychology.

At this remove, none of the problems that would follow from an increased disciplinary role for very large research efforts seems as formidable as the obstacles that stand in the way of that increase. The subsequent problems will complicate the lives of those directly engaged in research, and it will take some major changes in our workways and our professional norms to resolve these and related problems. Nevertheless, the costs of this kind of problem solving would appear to be insignificant compared to the benefits that would accrue to the entire discipline if it were instituted.

Political science has the intellectual capacity and the methodological and technical competency to make a massive contribution to the welfare of the nation while evolving into a conceptually coherent scholarly enterprise. Knowing many of the colleagues who will be engaged in creating a new era for our discipline, I am quietly confident, and I think realistically confident, that to dream the appropriate dream is not to dream the impossible dream.

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“Citizenship” in “Industry”: The Case of Georges Sorel

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Georges Sorel was of course a critic of "citizenship" in the sense current then as now. But behind his ideal picture of industrial organization there lay a conception of citizenship which derived from an older tradition. This conception in turn modified the themes which Sorel drew from a second tradition, that of thought about "industry." The result was a vision of industrial organization which displaced civil association by taking some of its typical features for itself. What is represented by notions such as the "myth of the general strike" is not, as some have held, a romantic reaction against modern organization but, on the contrary, a thoroughly modern doctrine of organization as the "sublimation" of civil life which continues to pose problems of relevance today.

In the concluding chapter of his *Politics and Vision* (1960), Sheldon Wolin regrets the contemporary substitution of "organization" for "politics." The individual's attachments and concerns, as pictured in modern social theory, have been deflected from the traditional sites of citizenship to the enclosed and particular sites of corporate life, which, Wolin suggests, have thus come to assume for themselves themes once reserved to the polity itself. In this process of "sublimation," however, the idea of citizenship has necessarily lost a crucial part of its meaning: what a particular association cannot do is to provide its members with that sense of *generality* which the polity, as it was once understood, ideally supplied. Wolin writes: "To contend that individual participation can be satisfied in a political way within the confines of nonpolitical groups is to deprive citizenship of its meaning and to render political loyalty impossible. When used in a political sense, citizenship and loyalty have meaning only in reference to a general order" (Wolin, 1960, p. 433).

Georges Sorel appears no more than fleetingly in Wolin's account, and then only as an example —together with rebellious critics such as Kierkegaard and Nietzsche—of the "last-ditch efforts to secure some place for unorganized individual action," or of the "last gasps of a romanticism doomed to expire before the age of streamlined organizations" (p. 423). His work has such a character, it is claimed, by virtue of that "heroic impulse" which he perceived in the myth of the general strike, through which the industrial proletariat was to achieve a spontaneous and *un*organized unity. Now such a view of Sorel as essentially a romantic outsider is by no means uncommon; but I believe it is wrong, and, in Wolin's case, so *precisely* wrong that it is worth correcting. Far from exemplifying the last gasp of a romanticism doomed to failure by organization, Sorel is among the most striking and interestingly complex examples of Wolin's "sublimation of politics" itself. It is in his writings, more clearly and relevantly than in any others of which I am aware, that one can detect the gradual absorption of a classical picture of citizenship by an ideal of industrial organization. If there is a single developmental point in modern political thinking at which the idea of citizenship began to lose its traditional moorings in the polity, it is in Sorel that we are likely to find it.

There is more to this question, however, than the correction of what amounts to little more than a footnote in Wolin. For Wolin's "sublimation" thesis confronts another well-developed and forceful recent theme, one which is directly contrary in its logic. According to Wolin, the migration of citizenship from polity to enterprise necessarily represents a loss; for other theorists of democracy, from G. D. H. Cole to the present, it represents a clear gain in reality, since, they maintain, a citizenship reserved to the polity can be no more than a fiction. Cole wrote: "The Guild Socialist conception of democracy, which it assumes to be good, involves an active and not merely a passive citizenship on the part of the members. Moreover, and this is perhaps the most vital and significant assumption of all, it regards this democratic principle as applying, not only or mainly to some special sphere of social action known as 'politics,' but to any and every form of social action, and, in especial, to industrial and economic fully as much as to political affairs" (1920, p. 12). Later writings in this vein have often been severely critical of those who, like Wolin, have argued for an essential difference between general and particular contexts of action; in ascribing reality to an illusory "general" order standing over and above the concrete and specific relations of productive life, Wolin, it is contended, is guilty of "reification" (Pateman, 1975). In all of this there are hints, of course, of Marx, whose essay "On the Jewish Question" is sometimes invoked; but Sorel, none of whose writings is ever invoked by theorists of citizenship in industry, anticipated some themes of that school with rather more precision, in seeking to transpose the realm of citizenship rather than (as in Marx) to obliterate or transcend the category. Indeed, if the term had not been put to other uses, and made to stand—misleadingly—for a voluntarist and romantic mood, one might well describe as "Sorelian" all those trends of recent thinking in which citizenship has been located in the industrial realm. In examining the paths of thinking which led Sorel himself to such a vision, we may arrive at some general suggestions about the coherence of the vision itself.

The Citizen, the Warrior, and the Worker

How, though, does the idea of citizenship enter into Sorel's central concerns? Paradoxically, perhaps, it does so by way of a conception which, on the face of it, eliminates citizenship altogether: that of an "industrial society." What Sorel (and his disciple (Edouard Berth) understood by *la société industrielle* was an order following the organizational contours of industry itself, in which no political structure mediated the relations between one productive grouping and another, professional ties replacing civil ones as objects of loyalty. Sorel and Berth were not the originators of such a conception, whose beginnings are often seen in Saint-Simon; but the use which they made of it lent it a quite new edge, and brought into play a most interesting and unexpected convergence of disparate themes. While Saint-Simon had contrasted a society organized on industrial principles with an aristocratic or ascriptive society, Sorel and Berth contrasted it, rather, with a commercial society, *la société marchande* (Sorel, 1935, p. 67). In offering this contrast, they implicitly link hands with an older tradition which likewise had often concerned itself with the relations between "commerce" and "virtue" (Pocock, 1972; Horne, 1978): the civic tradition in political thought.[1]

Montesquieu, Hume and de Tocqueville, among many others, had noted the contradiction between the classical ideal of the self-repression and self-forgetfulness of the citizen and a social order in which the predominant motive was profit-seeking (Richter, 1970). While some—including all three of those named—had resolved or eased this contradiction to their own satisfaction, offering grounds for supposing that citizenship could indeed be preserved and fostered in a society of "merchants," they had in doing so perpetuated the long-standing distinction between a life of business and civic life; in particular, they and others had done so in contrasting the essentially military virtues of the classical citizen with the pacific values of *le doux commerce.* And here one can hardly fail to be struck by the continuity of Sorel's argument with that of these earlier theorists. For, in the first place, Sorel expressly (and unfavorably) contrasted the commercial bourgeoisie of his day with the citizen class of Athens: "The ancient Athenians were much superior to our envious, ignorant and greedy bourgeoisie. . . . The citizens were not merchants, demanding guarantees for their transactions and protection for their industry, or seeking favours from government. They were soldiers whose very life was linked to the greatness of the city. The slightest weakness placed the state in danger" (1889, p. 172). Second, as this quotation itself indicates, the idea of loyalty and devotion, conceived of upon an essentially military pattern, was quite fundamental to Sorel's conception of order: and in proposing to replace the "envious, ignorant and greedy bourgeoisie" with a society of industrial producers, Sorel also assimilated the latter to a soldierly image—"Socialism returns to ancient thinking: but the warrior of the city has become the worker of advanced industry; weapons have been replaced by machines" (Sorel, 1933, p. 311). It was, of course, the supposed warrior-like character of the syndicalist worker that Sorel was to take as his starting-point in his notorious defence of violence and of "class *war*"—an expression which, as the text of *Réflexions sur la violence* makes very clear, is no mere synonym for generic conflict or struggle, but is to be taken quite literally (Sorel, 1930, pp. 434—35). The syndicalist worker represented the citizen in Sorel's mind, while the bourgeois represented the citizen's negation. Austerity and self-abnegation are contrasted with frivolity, luxury and dependence; and it is this essentially moral contrast, so characteristic of a long-standing tradition of civic thinking, that figures in Sorel's picturing of the relations between worker and bourgeois, rather than any Marxian thesis of exploitation, in which Sorel displays little if any interest.

The essential connection between war and citizenship is evident as early as 1889 in Sorel's remarkable book, *Le Procès de Socrate,* in which Socratic philosophy is condemned, as it had been by Socrates' prosecutors, for its damaging effects upon tradition and convention, by which alone the intense solidarity of the city-state could have been maintained. Convention unites: philosophy divides, for dialectical skills are very unequally distributed, and thus separate the philosophical few from the unphilosophical many (1889, p. 176). That, in essence, is Sorel's radical critique of Socrates, who, he contends, had cast potentially ruinous doubt upon the unquestioning loyalties and faithfulness to routine upon which classical citizenship depended. "Socrates strove mightily to break the chains which bound the citizen to the ancient city. These chains were those of military discipline. The citizen was a soldier, closely confined and watched; he was obliged to submit himself to a system of education designed to integrate him and to prepare him well for war" (1889, pp. 6-7). This education was provided above all by a civic poetry and drama; and it is interesting to note that exactly these vehicles of civic education were to reappear, transformed, in Sorel's socialist phase, as the "social poetry" of Marx (1919, p. 189) and as the "drama" (1930, p. 173) of the general strike, the *educative* force of both of which is stressed especially. Moreover, even in *Le Procès de Socrate,* which displays little interest in, let alone any inclination towards, the socialist position, Sorel was already disposed to efface any clear distinction between the realms of war and of work, noting approvingly the military analogies for production in Xenophon's *Oeconomicus* (1889, pp. 378-79): a theme which, as we shall see, was later to be very amply developed.

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[1] In Pocock's discussion it is the *rentier* rather than the merchant who emerges as the enemy of "virtue," for "independent proprietorship" of land *or* of capital was accepted, he shows, as a qualification for citizenship. But it was evidently Sorel's view (1889, p. 172) that the merchant lacked the crucial quality of "independence."

When we turn to Sorel's conception of contemporary citizenship, we find that here too essentially military themes are stressed. In *Le Procès de Socrate* he had written: "It is impossible to understand the ancient constitutions without relating them to military institutions" (1889, p. 167). Likewise, he later insists that the political spirit of Republican France draws what force it has from "military institutions": with the rediscovery of the citizen army by the French revolutionaries, "a quite new notion of the City was born, with strong analogies to that of antiquity, and patriotism became a force of hitherto unsuspected importance. Democracy still lives off this tradition" (1933, pp. 317-18). And as before, this living citizenship, military and patriotic as was that of antiquity, is sharply contrasted with a vacuous and rationalistic understanding of the city. What the revolutionaries *did* gave birth to a new order; what their theorists *said* displayed no understanding whatsoever of the nature of order. "Once the *believer* of the Church, then the *subject* of the King, man was to feel his strength and become a *citizen*; but what is to be understood by this term? Theorists asked what would be left in man after the ties subjecting him to Church and King had been dissolved; this residue was natural man, an abstraction formed out of the virtues and faults of eighteenth-century educated man." This abstraction could be made real only by the application of political power: "Discussion was continued in the manner of the old political theorists; the doctrine which summed up the fundamental principles of government was taken to be of prime importance" (1902, pp. 6-7). The French Republican tradition rested, therefore, upon a wholly deluded self-understanding in which the real sources of civic attachment are overlooked in favor of "abstraction" and political "doctrine," abstractions and doctrines no less dry, corrosive and uncreative than those for which Athens, and Sorel, had condemned Socrates.

It is with the true source of citizenship, the military and patriotic consciousness of the citizen army, that Sorel later expressly links the syndicalist movement. It is the revolutionary syndicalists, he maintains, "who represent at the present time the spirit of the revolutionary warriors who, against all the rules of war, so thoroughly thrashed the fine armies" of France's enemies (1930, p. 376). Nothing could make it plainer that the substance of the city, in Sorel's view, has been wholly severed from the political realm, and that the concrete experience of citizen-like devotion and solidarity is no longer to be found in the official manifestations of *civisme* but in the industrially based institutions which stand in opposition to them. By this device, Sorel transforms the theme of civic solidarity into a thesis of class war. Where theorists of civic virtue had seen integrity in the transcendence or forgetfulness of particularity, and in the recognition of an interest of a general kind,[1] Sorel locates integrity in the class identification displayed in and reinforced by an internal war. Inter-class solidarity, as he came to say, was "hypocritical" (1911, p. 21), resting as it did, he believed, upon self-interested collusions and not upon any vivid communal or cooperative experience capable of bringing into being a passionate and unreflective loyalty.

But Sorel's historical imagery may seem very imperfectly lucid, for in his description of the revolutionary syndicalist movement he also employs an apparently contradictory analogy. He provocatively describes syndicalist militancy as "barbarous," and thus appears to invite us to imagine the revolutionary movement not in the light of the ancient city but in that of the barbarian invaders who destroyed it. This provocation has not been lost on his interpreters, among whom Wolin himself draws heavily upon it (1960, p. 223); and certainly, on the surface, it lends plausibility to the reading of Sorel as a theorist of sheer *élan.* But it should not be overlooked that in the civic tradition the distance between ancient citizen and barbarian invader was much closer than mere historical fact would suggest. As Pocock has reminded us, theorists of civic virtue had appealed at once to "Spartan," "Roman" *and* "Gothic" images (Pocock, 1960, pp. 94-95). Sorel's "Goths," too, are essentially orderly beings, more civic in fact than the citizens themselves;[3] they brought new forms of juridical and social order to a world which had forgotten the "quiritary virtues" (Sorel, 1922, p. iv). They did not destroy the city, for, as Sorel explains at length in *La Ruine du monde antique,* there was no city left to destroy, its spirit having been ruined by processes of internal corruption. Likewise, the syndicalists, as heirs to the "barbarians," bring to the world a new source of *droit,* which is to be found in the practical cooperation and solidarity of the factory, and not in the patched-up compromises of interclass relations upon which the present city expends its declining energies (1919, p. 73 n.). To the extent, then, that Sorel regards the syndicalist movement as a movement of barbarians rather than of citizens, he does so just to the extent that his picture of the barbarians contained in advance that theme of the civic spirit's migration to a new bearer, a theme to which his syndicalist writings were to give a new application and a fuller development.

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[1] There is, of course, Machiavelli's discussion of the benign effects of class conflict in the *Discourses* (I. iv). But Sorel's "classes" are not Machiavelli's (see Sorel, 1930, pp. 73-76), and conflict between them is designed to be terminal.

The Idea of Industry

One problem which Sorel confronted, therefore, was that of distinguishing the true *civisme* of a movement which he admired from the sham citizenship of an order which enjoyed the symbolism and formal trappings of a city. The problem was not new: one might compare Cicero's remark, "Through our vices we retain only the name of a republic, and have long since lost the reality" (see also St. Augustine, *City of God* II. 21, where Cicero is quoted). Sorel likewise contrasted the "vices" of the city-in-name, especially the "voracious appetite" (1930, p. 221) or political passions and ambitions of its inhabitants, with the self-repression and devotion to common ends which the idea of citizenship demanded. A second problem, to which we must now turn, required a more novel adaptation of tradition. In seeking to locate citizenship within the realm of industrial life, Sorel was attempting to put that idea to a historically incongruous use. While his dislike of commercial society was wholly traditional, his admiration for industrial society was not; and in seeking to enforce a radical distinction between production and trade (1902, p. 12), investing the former with virtue, the latter with its corruption, Sorel was doing something almost wholly unprepared for in the civic tradition, whose exponents—from Aristotle on—had been no more inclined to value the producer than the merchant. Here Sorel's thinking brings into play a quite different train of antecedents, many of whose themes had been in some respects profoundly opposed to those of the classical doctrines of citizenship.

As Michael James has shown in an excellent article (1977), the immediate roots of the notion of *industrie* are to be found in eighteenth-century political economists such as Pierre-Louis Roederer and Jean-Baptiste Say, who sought to establish the managerial, productive and inventive skills of the *industrieux* as a factor of production distinct from capital, labor and land. If their principal target was the Physicratic doctrine that land was the primary source of wealth, they were led also to dispute the doctrine that it was the capital of the entrepreneur that constituted his productive function, and also the labor theory of John Locke, for it was not only the *peine* (labor) which one suffered in acquisition but the *adresse* (skill) which one exerted that constituted a property title. *Industrie* no less than land or capital was something in which one could have *propriété,* and the *propriétaires d'industrie* therefore rightfully deserved the citizenship traditionally attached to property, and should be admitted to political participation. It was this that lent the notion of *industrie* its ideological edge: it figured in a demand for the extension of the franchise to the *industrieux,* by establishing *industrie* as a property.

Industrie therefore emerged initially as a criterion of *civisme* or of inclusion in the civic body, a criterion which fundamentally challenged the Aristotelian view (*Politics,* I. 7) that the direction of labor enjoyed no higher a status than labor itself. But it is the further extension of this argument that has a particular relevance here. Initially conceived of in terms of the Aristotelian model of inclusion—who should participate?—the idea of *industrie* passed over to revising Plato, in proposing *les industriels* as the exclusively legitimate bearers of authority, by virtue of the crucial importance and privileged character of their special knowledge and skill. It is in Saint-Simon's celebrated "Parable" of 1819 that this transition is most clearly revealed. At first a liberal who sought to modify the criteria of citizenship and to include *les industriels* within the polity, Saint-Simon eventually moved to a radical demand—"socialist," or "managerialist," or somewhere in between the two—that industrial, technological and creative competence should supersede all other criteria of membership. In the later Saint-Simon, as also in Comte (who added to the doctrine some features peculiarly his own) what began as a project of political reform ends up as the far-reaching enterprise of founding a new social order upon the basis of a newly recognized principle of capacity. In his "Parable," Saint-Simon imagines the sudden elimination of France's present governing class, as a mental experiment designed to convey the uselessness of ascribed status and unearned wealth; how marginal would be the impact of this loss, he says, compared with the devastation that would be caused by the sudden loss of an equal number of scientists, artists, and artisans. The passage is striking not only because (as one commentator has put it) it is a "concise and vivid expression of the theme of an industrial society" (Markham, 1964, p. xxvi), but also because it expresses so clearly the transition from the notion of *estate* (defined by the nature *of* what one does) to the modern notion of *élite* (defined by capacity *in* what one does); for the qualities of *industrie* are, it seems, concentrated disproportionately in the hands and minds of a few thousands of *industriels*. Increasingly, in both Saint-Simon and Comte, the idea of the *industriel* comes to be separated from that of the *prolétaire*.

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[3] It is no surprise that Sorel (1933, p. 138) should have found Macaulay's notion of revolutionary "paganism" appealing, for "pagan" embraces "Roman" and "Gothic" in an ideologically convenient fashion.

"Industrial capacity, that of the arts and professions, is what must be substituted for feudal or military power," Comte wrote (1929, Appendix, p. 64). This theme, common to both Saint-Simon and Comte, is explained by Comte as follows: "There are only two ends that may be sought by a society, however large, just as there are for a single individual. These are violent action directed against the rest of mankind, or conquest, and action upon nature in order to modify it to human advantage, or production. Any society which is not clearly organized for one or other end will be nothing but a hybrid and characterless association. The military end was that of the old system, the industrial end is that of the new." The pregnancy of this remark is evident, for the military organization of the city had been essentially tied, in a long tradition of political thinking, to the very notion of citizenship; bearing arms for the city was conceived of both as an expression of and a prerequisite to full membership in it, as a city necessarily confronts other cities, and the valuing of its identity implies the readiness to defend it. It is no surprise to find, then, that in the industrial and pacific vision, as it reaches its epitome in Comte, it is no longer the city, nor even a state imagined as the bearer of city-like properties, that is to be the primary focus of loyalty: it is the town, *commune,* imagined as an essentially social and productive entity linked pacifically to other such entities, by ties of economic interdependence, on a European and eventually a global scale. This common "action upon nature," as Comte calls it, supplies them with a shared interest, an interest institutionally embodied in their acceptance of the authority of an elite of savants, of scientists and sociologists whose universal "theory" is a necessary foundation for the local "practices" of productive life.

Now Sorel despised Comte and his disciples with real vehemence. What provoked him was in part the positivist view of history as the march of enlightenment, for Sorel was a shrewd methodologist and hence a sharp critic of notions of inexorable historical progress, on grounds that have since become quite familiar. What also provoked him was Comte's view that practice was the child of theory, that technology depends essentially upon a chain of abstract sciences which are the province of the savant, and which find their ultimate expression in a set of rationally connected and stable theoretical doctrines. In truth, Sorel believed, science is the child of technology, and the theoretical tenets of science are no more than abstractions performed upon the concrete operations of the factory (1894, pp. 201-11). It is therefore no accident that Comte (1975, Vol. 2, p. 73) should have called for the harmony of "hands" and "heads," while Sorel (1908, p. 60) called for a "philosophy of the hands" which would exclude a "philosophy of the head." But Sorel's evident rejection of the Comtrean tradition, as of the Saint-Simonians—whom he called "bookworms" —should not blind us to the fact that what he was offering was not at all a rejection of the idea of *industrie* but an internal modification of it. Saint-Simon may or may not have been a socialist, Comte was not; but when Sorel defines a socialist order as one which is "organized according to the requirements of production," he is clearly echoing the theorists of *industrie* in constructing his picture of socialism. Sorel's socialism, no less than Comte's technocracy, was motivated by a dislike for the political class in general, and for lawyers in particular—for abstract and manipulative skills which do nothing, as Sorel put it, to keep the factories running; and the ties binding one *polytechnicien* to another, one suspects, go deeper than mere ideology.

What made Sorel's picture socialist was precisely the fact that he differed from Saint-Simon and Comte with respect to the identity of the *propriétaires d'industrie*; in Sorel's view, which is certainly eccentric, and which may very well be plain wrong, the conditions of avanced technology call for a wider and wider diffusion of skill and technical knowledge (1947, pp. 282-83), so that, in effect, the criterion of *industrie* tends to become increasingly inclusive, embracing those whom earlier theorists had called *prolétaires.* Nor can there be much doubt, despite the fact that Sorel described himself with perhaps surprising persistence as a Marxist, that it was this French tradition, rather than a Marxian one, that lends intelligibility to what he wrote. For whereas Marx primarily stressed the dispossession of the proletariat, its reduction to bare laboring humanity (*peine*), Sorel stressed above all its acquisition of technical capacity (*adresse*), hence not its dispossession but its proprietorship of productive skills. In fact, in a most intriguing passage, Sorel suggests that property itself is valuable only as a medium for the development of skill (1947, pp. 282-83) and he thus shifts the focus of concern from the Marxian question of ownership to the question of the opportunities presented by the work process.

Sorel's belief that modern industry enhanced the skills and technical capacity of the workers is so odd a view, and cuts so sharply across a powerful and familiar trend of social criticism, that we may well speculate about the reasons for it. Here Comte's rigorous distinction between what he called "theory" and "practice," between the hierarchy of abstract sciences and the loyal but merely laborious application of the worker, may provide a valuable clue. Such a distinction, which Sorel found so offensive, recalls the central theme of his early book on Socrates, who, Sorel maintained, had ruined the ancient city by separating the able and knowledgeable thinker from the simple and unreflective doer. By separating thinking from doing, he had destroyed the civic unity founded upon the shared central symbols expressed in poetry and drama; the city's judgment against Socrates (technicalities apart) was in the last resort correct, for Socratic philosophy was indeed destructive of the city. It is surely not fanciful to suggest that Sorel's critique of Comte should be read in the light of his earlier critique of Socrates. The technocratic division of industrial society into scientists and managers on the one hand and laborers on the other echoes the Socratic division of the city into philosophers on the one hand and warriors and artisans on the other. In criticizing Saint-Simon and Comte, Sorel was recalling implicitly an archaic vision of the city, with an undivided life and a common end; and we may speculate that his astonishing picture of the industrial workplace expressed his need to find a contemporary bearer for this longstanding image.

Comte, moreover, had contrasted "conquest," the "violent action" of a city "directed against the rest of mankind," with "production," or "action upon nature in order to modify it to human advantage." It is of the greatest possible significance that where Comte spoke of "action upon nature" in contradistinction to "conquest," Sorel spoke, rather, of the conquest of nature (1930, p. 96), bringing to the technological exploitation of nature a language strongly infused with military images. Sorel imagined "men" and "nature" as antithetical, men's efforts being constantly thwarted and obstructed by the resistance of the natural world. The producers' virtues are citizen-like, too, not only in their belligerence but in the fact that they display that "excellence in the mass" of which Aristotle had spoken; they require heroic solidarity, not heroic personality, and learned practical competence, not esoteric knowledge. It was in part the reminiscence of this image that led Sorel to depart so sharply from the Saint-Simonian or Comtean picture of science and society, just as it intensified his distaste—itself inherited from such as Saint-Simon and Comte—for the elite of orators and intriguers who were ruining France just as they had once ruined Athens. And with Sorel's adaptation of the notion of *industrie,* that notion undergoes a further transformation: at first a criterion for citizenship (in Roederer and Say), and then a substitute for citizenship (in Saint-Simon and Comte), it eventually becomes a new model of citizenship, the industrial world bearing all the idealized properties of the city itself.

These idealized properties, as a necessary corollary, are drained out of the *political* city, which comes to bear all the antitheses of order. It is evident that this logic points strongly towards the images of war which were to figure so prominently in *Reflections on Violence,* for it tended increasingly to assimilate the relations between classes to the relations between hostile cities. Revolution becomes internal war. It is no longer a transition of the kind imagined by earlier theorists of *industrie* such as Comte, a developmental process undergone by a whole society, in the course of which all the parts of an order are operated upon by factors which eventually culminate in a new and total way of life and thought: it cannot have this character, for a *city,* unlike a historical tendency, requires acknowledgment and loyalty, and cannot be the cumulative product of successive unwitting contributions. Nor, on the other hand, is revolution a catastrophe, an extraordinary and destructive event *after* which the form of a new order of things will eventually come to light; it cannot have this character either, for a war presupposes an already-existing recognition of solidarity and identity (Vernon, 1978, pp. 23-37). In opposition to both of these broad conceptions of transition, Sorel insists that a revolution is the displacement of one order by another which has already developed its institutions and loyalties, and that the *civisme* of the new order should have emerged in advance among the members of the insurgent movement.

The Sublimation of the City

The centerpiece of Sorel's revolutionary syndicalist thinking is of course the "myth of the general strike": and the analysis sketched above should help to shed light upon the political meaning of this construct, which may indeed be seen as the culmination of Sorel's effort to transpose the values of *civisme* to the world of *industrie.* Much attention has been given by Sorel's commentators to its "mythic" character; much less attention has been given to its equally significant "general" character. But in both respects, its features evidently derive from what we may call the civic dimension of Sorel's thinking. The idea of myth has an altogether incongruous place among the highly rationalized conceptions of *industrie,* which, according to Sorel, brings about the progressive extension of what is made at the expense of what is given, and thus inexorably reduces the realm of mystery. In this respect it belongs to Vico's "third age" of "men," and not to the mythic ages of "gods" or even of "heroes" (Vico, 1970, pp. 285-98). Sorel's civic theme, however, allows an ample and quite fundamental role to myth, for, he writes, "It is impossible to express ideas about the *patrie* except in a mythical form" (1933, p. 245). "Generality" belongs likewise—as Wolin stresses—to the civic ideal; and we may suggest tentatively that its application to the claims and imagery of the socialist movement forms an essential part of Sorel's effort to understand an industrial organization as a city.

During the 1890s, Sorel had espoused a highly individual but not wholly unorthodox social-democratic position, which allowed a significant place for political reform and for the political institutions permitting reform to be pursued. After the turn of the century, as is well known, he abandoned this position for a version of socialism in which all political instrumentalities were condemned root-and-branch, and everything made to hinge upon the autonomous or even perhaps antinomian development of a revolutionary movement. This transition is most economically viewed in terms of the shifting relations which Sorel perceived between the two cities with which he was preoccupied. The Augustinian echoes of this phrase are not out of place: for, like Augustine, Sorel stressed the fact that the two cities, the true and the false, were "mixed" together (1919, p. 17), their members—though destined for different ends—spatially interspersed and highly interdependent. Indeed, it is only by a "mental effort" (1930, p. 189), or by a process of abstraction which Sorel calls *diremption* (1930, p. 407), that the lines of demarcation between the combatants can be clearly grasped, so manifold and concrete are the ties between one and the other. Though Sorel's cities, like Augustine's, have different "times," different ideal schemes of beginning and destination, the industrial city being defined by its "socialist future" (1919, pp. 77-167), the two share for the time being a common "history." What is still more to the point is that Sorel admits, as Augustine did, that the false city may offer a field of experience in which the loyalties of the true city are tested, and also an analogy or model through which the virtues of the true city may be taught. In the writings of his social-democratic phase, Sorel did not claim anything more than that for political participation in Third Republic France. When, in the course of the Dreyfus affair especially, he had advocated political involvement on the part of the socialist movement, he had done so on the grounds that such involvement would stimulate and render habitual a sense of justice of crucial long-term importance to the movement itself. "The only manner in which we may cultivate a sentiment is by exercising it whenever an opportunity presents itself. Since we propose the effacement of classes, we must exercise the sentiments of justice, especially in circumstances in which they conflict with class divisions. In fact, we cannot be sure that we are pursuing justice unless we are quite certain that we are disinterested" (1898, pp. xv-xvi).

What justifies political action by the socialist movement, therefore, is the resemblance between the struggles internal to the political city and those to be waged against it, a resemblance which is sufficiently strong that participation in the one reinforces commitment to the other. At this stage, indeed, Sorel is prepared to see both as involving the cause of a universal principle of "justice," a notion which at that point makes one of its rather rare incursions into Sorel's argument. Ten years later, all such appeals to principles or ideals supposedly transcending class boundaries are roundly condemned, and commitment to the struggle against the Third Republic is held to exclude all participation in the struggles internal to it. But there is no fundamental change in the structure of Sorel's thinking here, for it is the *resemblance* of one city to the other that now displays a second facet of its ambiguous potential. Resemblance may supply an argument for continuity, the experience of action within the political city coinciding with and reinforcing the ideals which are eventually to lead to its destruction: or it may supply an argument for displacement, the two cities posing a choice precisely because of their resemblance as orders with sovereign claims, which excludes all accommodation between the two. As this second version of the argument evolves in Sorel's thinking, we find the language of civic loyalty penetrating his discussion still more deeply. The working class becomes "a people" (1902, p. 28); the institutions of the Third Republic become "foreign" to it (1919, p. 69); and, most notably, the notion of war against another class is added to the metaphorical war against nature in which the industrial city is said to be engaged.

This new "people" is no longer to form and model itself by participation *in* the institutions of the city which confronts it, but by participation in an act of revolutionary opposition *to* them. Nevertheless, it is still in the city-like activity of war that the movement is to define itself, and to that extent Sorel's new strategy still insists upon the need for the city of industry to draw upon the logic of the political city in order to disentangle itself from it. And just as, in the manner of other sovereign entities, it must define itself by war, so too, in the manner of other sovereign entities, it must define and found itself by myth, a conception which, in the history of political thought, bears an especially close relation to the question of beginning or founding (Tudor, 1972). The loyalties upon which an established city depends are secured by the force of convention or tradition transmitted over time; but a process of transmission requires a beginning, and here myth seems a necessary complement to tradition, for there must have been a time when the loyalties later to be sustained by convention and habit required to be sustained by vivid prospective anticipations, and by epic events which were to reveal their full meaning only with the passage of time (Pocock, 1960, pp. 242-43). Sorel's myth is above all a foundation myth, describing events in the light of which an order is to understand its identity, and from which later generations will draw their traditions of understanding and behavior (1930, p. 436). As in his social-democratic phase, Sorel's central concern is still with "the genesis of morality," with providing the institutional and psychological conditions for that spirit of devoted self-sacrifice upon which the new order will eventually rely.

What is meant by the "irrationality" of Sorel's myth, therefore, may need to be revised. Its irrationality is not, as some have supposed, that of a romantic gesture, an attempt to keep the flame of commitment burning without regard to the consequences of doing so: Sorel's approach is thoroughly (and ambitiously) consequentialist in its assessment of the manner in which institutions and practices exercise their long-term impact upon belief and action. Its irrationality would appear to be, rather, that residual irrationality of the city itself, a feature which has migrated to industrial organization along with the other features of *civisme.* Simply because it is a bounded and exclusive entity, a city draws limits to the scope of reason: it accepts an intensity of violence in its external relations which it cannot tolerate internally; and it must take as given the accidents, the force, and the sentiments, prejudices or legends which make it one city, for its boundaries are not determined by anything more than that. The apparent romanticism of Sorel's argument, one suspects, springs wholly from the fact that it transfers claims from a context in which we are accustomed to see them to a context in which we do not expect them to appear. His warrior-like syndicalists, unified by a colorful epic of struggle and triumph, parody the idea of the city; but like all parodies, Sorel's picture retains some significant faithfulness to the original.

As for the "generality" of the mythic strike, may we not suspect here, too, a parody? The *grève générale* displaces, yet also imitates, the *volonté générale* of citizenship. Like Rousseau, who saw in the "art of generalizing" the ability to link particular with civil ends, so too Sorel values the general strike as the means of overcoming local particularity of interest. The workers' movement, as he insisted, rested upon essentially local organizations, upon the networks of personal and professional ties among the inhabitants of provincial towns (1902, pp. 27-28); and the function of the idea of the general strike is precisely to link such complexes of local interest and attachment to a nationwide movement. "Strikes would be no more than simply economic events of negligible social significance," he wrote, "if the revolutionaries did not intervene, changing their character and making them episodes in the social struggle. Each strike, however local, is a skirmish in the great battle known as the general strike. The association of ideas here is so simple that one has only to point it out to striking workers in order to make them socialists" (1919, pp. 60-61). By virtue of this "association of ideas," "each," in Rousseau's words, is to include himself in the "all," so that the ends of all are motivated by the interests of each. For Sorel, no less than Rousseau, was worried by the menace of particularity, and also by that of the "will of all" which substitutes a self-interested coalition of forces for the expression of the general ends of the movement (1919, p. 69). And even more like Rousseau, Sorel insisted that the solution to this problem lay in the effective organization of communal will, not in subordination to government; just as an effective *volonté générale* is to render government a mere instrument of a community made one by its own consciousness, so too the *grève générale* is intended above all as an alternative to the imposed and artificial unity of bureaucratic organization: syndicalism is to reduce "central committees" to mere "administrative offices" (1902, p. 26).

But the critique of bureaucratic organization does not imply a critique of organization as such. On the contrary, what is eventually to substitute itself for the artificial organization of government

—or for that of political socialism—is the organization of industrial production itself. The Rousseauean general will of citizenship collapses into the "generality" of production, which is, Sorel says, *générale, anonyme, inconscient* (1933, p. 272). It is "general" in that each producer figures only as a small part of an immense and interdependent order, whose time is constituted by the patient, methodical and cumulative progress of achievement, the scale of which altogether exceeds individual control. It is "anonymous" in that the producers—like Rousseau's citizens, who recognize that any law bears equally upon themselves and others—are interchangeable, each filling a role which another might fill equally well. Since it is also "unconscious," it requires for the time being an organization of consciousness which draws directly upon the model of the city, a class war which transposes to the workers' movement the themes and images of the political city itself: the myth of the citizen-army, of the revolutionary *levée en masse,* is to be detached from the Jacobin tradition and attached to the socialist movement. But after victory, as class war in turn fades into memory and legend, it is the endless struggle against nature that is to supply the new order with its organizational principles, and the "generality" of productive organization is to displace the generality of will.

Citizenship in Industry?

Wolin views Sorel as a romantic opponent of the organization which has displaced the city. What has been suggested here is that, on the contrary, Sorel romantically reduced the city to an organization. Any notion that he revered the unorganized should rapidly be dispelled by this brief account of what he took the perfected industrial order to be like:

> It must be understood that the socialist factory will be a grouping of producers who will always be on the alert in criticizing received practices, who will be guided by supervisors comparable to the demonstrators employed by professors of chemistry, and at the head of whom will be engineers who will speak to their men as a master speaks to his students (1947, pp. 354-55).

Whatever this is, it is not a cult of spontaneity. On the contrary, it is connected with a long-standing view of Sorel's that the strictest control of the individual by order does nothing to diminish freedom (1894, p. 179). That view was itself an element in his classical and civic reminiscences—as he continued to make clear even in his last writings (1921, p. 432)—in which it was the citizen's devoted self-abnegation that was especially stressed. I have tried to show above that, in the general logic of his thinking, his admiration of industrial organization sprang principally from a highly selective picture of the nature of citizenship: what he regarded as its fundamentally military character came to be connected in his mind with an equally selective picture of the nature of industrial work, and as these themes progressively fused the political city came increasingly to be dispensable, no longer of value even as an exemplification or reinforcement of moral experience. It is in terms of this fusion of themes, it was argued above, that Sorel's relation to both the civic tradition and Saint-Simonian "industrial society" should be understood; and it is in terms of this fusion, too, that the central concepts of his syndicalist writings acquire a degree of coherence, and that the nature of its irrationalism may be defined.

As noted above, however, the themes touched upon here may offer more than a revised interpretation of Sorel. What has been at the center of this discussion is Sorel's *pars pro toto* argument, and this is an argument which, as Wolin stresses, interestingly underpins so much of modern social and political thought. In particular, it is one which later theorists of participation and self-management have emphatically reinforced, in taking the industrial enterprise to be an analogue of the polity itself. To be sure, a discussion of Sorel's position can shed light only indirectly upon that of later theorists who have not shared in many of Sorel's central assumptions. But the general theme which Sorel played a neglected part in introducing has very far-reaching implications.

Sorel, as we have seen, drew attention to certain resemblances between the (industrial) part and the (civic) whole. Here he has been followed by later theorists (Cole, 1920; Bachrach, 1967; Pateman, 1970) to the extent that they too have denied that any qualitative gulf exists between the values attached to the citizenship available at the level of political institutions and the values which may be taught and learned at the level of the industrial enterprise. In several respects, it may be noted, Sorel has not been followed. First of all, Sorel spoke the classical language of self-repression or self-discipline, while the later writers have employed the more modern and appealing language of "self-development" and "self-realization." Second, Sorel distinguished between the "civic" and the "political," regarding the former with favor, the latter with contempt; later theorists, however, have entirely abandoned this distinction, investing the industrial enterprise with political no less than with civic properties. Third, the later writers have connected citizenship essentially with democracy, while Sorel—as his view of the socialist factory makes quite clear—did not. We shall consider shortly how much difference this makes to the theme at issue.

In Sorel's notion of the equivalence of industrial and civic identifications, we have observed a series of conflations or elisions. In particular, what has been elided is the distinction between the features of a thing and the definition of it. Participation in *industrie,* initially proposed as a criterion of membership in civil association, comes to define the nature of the city itself, and what began as an idea of qualification *for* citizenship comes to determine what citizenship *is*; what originated as a test becomes an analogy. Likewise, military capacity had once been viewed as a qualification for citizenship, serving either as a test or as a preparation for the enjoyment of political rights—those who fought *for* the city were entitled to be members *of* it. But Sorel's "war" against nature is not a preparatory but a definitive value, and it defined what the industrial city is in a manner in which war did not define what the political city was, however essential it may have been to it. In this respect, we may note, Sorel's proposed analogy between warrior-citizen and producer-citizen breaks down entirely, for the former is a citizen by virtue of being a warrior, the two statuses being distinguishable if inseparable, while the latter is citizen-like in being a producer, the two terms being merely interchangeable descriptions. Finally, we have seen that Sorel sought to make a transition from political to pre-political analogies: initially valuing the socialist movement for its capacity to participate in political struggles over issues of concern to a whole society, he had quite rapidly transferred his allegiance to a movement which rested upon an exclusive myth. Political struggles, as he contended, require "disinterestedness," the setting-aside of exclusively sectional attachment; myth, on the other hand, requires the identification of sectional and general ends, the setting-aside of particularity and the recognition of a common attachment now being accomplished within and not beyond the socialist movement. And the movement now retains its equivalence with the civic, not by virtue of expressing a comparable mode of action, but by virtue of resembling the city in its extra-political aspects, that is, in relying likewise upon violence and myth.

The consequences are revealed in Sorel's sketch of the factory of the future. The engineers are to "speak to their men as a master speaks to his students": the factory has become a school. It is not, however, a school of citizenship in the sense that what it teaches is preparatory to some further activity; it is not a school "of" *anything.* Here it contrasts with those other educational devices which Sorel had so admired in the ancient city—poetry, drama, myth and war—whose function was, after all, to equip the citizen for something else. Sorel has abstracted the experience of civic education from the ends which had made it "civic," and is thus led to his disturbing picture of a lifelong and intense preparation which is not, however, a preparation for anything. Now if all that concerns us here is the transmission of experiences of a certain kind, then indeed the relation between part and whole ceases to pose any problems at all. We can readily imagine that an experience of collective attachment could occur no less easily at one level than at the other, that such a value is wholly mobile between city and enterprise, and that differences among levels are, as it were, merely quantitative. But we may wonder if the notion of citizenship, thus understood, has any specific meaning left to it at all, and whether what is being discussed might not better be placed under the heading of moral education or personal development.

It is here that what needs to be said about Sorel may also need to be said in the context of later thinking. There can be no doubt that various attributes of the polity are also to be found elsewhere, whether we choose to stress (with Cole) a sense of responsibility, or (with Pateman) a sense of personal efficacy in the making of decisions which affect one, or (with Bachrach) the existence of power and authority relations, or the making of socially relevant decisions. But in these cases, no less than in Sorel's, one may wonder if the nature of a polity is altogether exhausted by analogical arguments; for analogy by its very nature is selective, and the resemblance of one institution to another—even in important respects—cannot establish their mutual substitutability. In those cases in which the analogy drawn bears upon the occurrence of some experience, the acquisition of some skill, or the learning of some attitude, we may indeed, as in Sorel's case, take this experience or skill or attitude to define what citizenship is; but then, as also in Sorel's case, we may wonder whether citizenship could not be exchanged for some other and more precisely descriptive term.

Unlike Sorel, as noted above, later theorists speak of "self-development," and also of "democracy" as a value. The idea of self-development may carry with it the implication that the attitudes or skills acquired in the context of the enterprise can then be applied by the individual elsewhere (Pateman, 1970, p. 110). Similarly, to speak of *democratic* citizenship is to introduce an appeal to a set of values which can and presumably should be brought to bear in any relevant context, and thus again to suggest that the principles learned in the enterprise may form the basis of action within, or reform of, a larger context of institutions. Unlike the various analogies offered above, such suggestions provide causal assertions which are open to empirical test, and do not simply assert a more or less plausible resemblance; the claim that what is learned or acquired in one context is subsequently transferred to another is, on the face of it at least, a convincing one, and one that has important implications for democratic theory. But this version or aspect of the participatory argument seems curiously at odds with the analogies which are held to support it. For if indeed an enterprise is an arena of citizenship, or a polity, why should it be so important that the experiences gained within it should be capable of transfer beyond? It is important that such a transfer should take place only to the extent that the further context of action is seen to be *un*like the context of original acquisition; for if the two were not in some respect different, the original experience would be complete (as Sorel, apparently, actually came to believe).

This conceptual difficulty is compounded by the transposition not only of "citizenship" but of "politics" itself from whole to part. Here, as noted above, later theorists have departed drastically from Sorel, who had no wish at all to claim the mantle of "politics" for the renewed "citizenship" of *industrie*. He evidently did not feel that the restriction of "politics" to the institutions of the state hampered in any way the cultivation of "civic" properties in the enterprise. The later theorists, however, dissent sharply, and take the view that their case for enhanced participation at the level of the enterprise hinges fundamentally upon the recognition of its "political" status. But this seems doubly unfortunate. If "citizenship" is to become a category of moral experience, then we clearly need another term for the category of relations among those institutions within which that experience occurs; and if "politics" follows "citizenship" along the Sorelian path of transposition, it is hard to see what term is left. If, on the other hand, the "political" character of the enterprise is taken to refer directly to its structural position—for example, to its impact upon general questions of distribution (Bachrach, 1967, pp. 80-81)—then once again the argument from analogy loses its point, for the internal similarity of enterprise and polity becomes irrelevant to the argument (Barnard and Vernon, 1975).

Sorel's contribution to modern political theory is usually regarded as malign. I have tried to show above that it is often condemned (or dismissed) for the wrong reasons, and that his central contribution bears a closer relation to some important current views and assumptions than is usually thought. He is the nearest and clearest ancestor of a transposition of language and of image which since his time has become increasingly pervasive. Perhaps, thanks to our emotional distance from Sorel, his writings offer an opportunity to assess not only the political but also the conceptual costs of his revised conception of what it means to be a citizen.

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Industrial Self-Management and Political Attitudes

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The purpose of this article is to test the widely shared view that the experience of industrial democracy, the experience of direct decision making at places of work, necessarily leads to the enhancement of cooperative and egalitarian orientations among participants. Based on data collected through in-depth interviews and questionnaires in Pacific-Northwest plywood cooperatives, the article argues that in market societies, such experiments in industrial democracy do not have the hypothesized results. Indeed, they seem to encourage the development of values most closely identified with classical liberalism or what MacPherson terms "possessive individualism."

It is widely believed that the experience of direct workplace democracy has profound effects on people's politico-social orientations and behaviors. More to the point, it is almost universally assumed that experience with cooperative and democratic procedures at the workplace, even in liberal-capitalist societies, tends to encourage the development of cooperative and democratic inclinations among people who are active in various forms of industrial democracy. This view is held by scholars and political commentators who are both friendly toward industrial democracy and those who are decidedly unfriendly.[1] The purpose of this article is to test the validity of this view by examining the observable effects of the experience of industrial democracy on people who are actively engaged in workplace decision making. Does the experience of cooperative and egalitarian forms of productive activity at work translate into the enhancement of cooperative and egalitarian values and behavior in a society (the United States) whose cultural benchmarks are individualism and competitiveness?

By far the most pronounced expression of the view that cooperative and egalitarian work relations nurture generalized cooperative and egalitarian values and behavior may be found among various shades of socialist opinion, both in the United States and abroad. In one variant of this view, industrial democracy represents an approximation of a humane socialism that might exist and even expand within the context of liberal-capitalist society (Bernstein, 1974; Blumberg, 1969; Cole, 1919, 1920; Pateman, 1970; Vanek, 1975). As such, industrial democracy is valued in its own right and as an embryonic vision of the good society. It offers the possibility of creating islands of partial socialism, defined as worker and community control of productive enterprises, within the very body of capitalist society. While the idea remains unstated, one gets the impression from this view that the transformation to socialism might be accomplished through a peaceful process in which workers and communities gradually take over an ever-larger proportion of enterprises.

A commitment to the most extensive form of industrial democracy, namely, complete workers' self-management, is now an integral part of the articulated program of virtually every socialist and communist party in the industrialized West. In the United States, groups like the New American Movement, The Democratic Socialist Organizing Committee and the Campaign for Economic Democracy have also embraced the principle of self-management. While I do not have space to explore the roots of this interest in self-management, it is clear that it is compounded out of a

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[1] A range of literature supports various forms of workplace democracy. Commitment to very limited forms may be found among business and political leaders disturbed by the so-called "productivity crisis" in the American economy, hoping that small-scale participatory reforms might serve to "cool-out" labor conflict, enhance work enthusiasm, and diminish absenteeism, turnover, and product call-backs (Gooding, 1972; HEW, 1973; Jenkins, 1974; Sheppard and Herrick, 1972). Industrial democracy is also advocated by some who perceive it to be an intrinsically superior form of human work existence. Humanistic psychologists like Argyris, Maslow, Horney, and Rogers have long called for new forms of work organization that more fully involve the worker in enterprise decision making as a way to transcend the alienation and stunted personality development all too typical of conventional work organization and to allow human beings to fully develop their powers of creativity, self-esteem, and self-actualization (Argyris, 1965; Horney, 1950; Maslow, 1965; Rogers, 1951). Another strong voice calling for industrial democracy comes from the participatory tradition in democratic theory, based on the contention that a democratic political life must necessarily be built upon the bedrock of a democratic citizenry trained in all of the major institutions of the social order (Bachrach, 1967; Bay, 1958; Blumberg, 1969; Kariel, 1970; Pateman, 1970).

complex interaction among the traditional Marxist analysis of alienated labor, the recurrent and seemingly spontaneous phenomenon of worker-councils and soviets appearing in the midst of virtually all twentieth-century social struggles, and the apparent poverty and inappropriateness of most of the traditional strategies of the left in the advanced countries. Out of this confusing combination of developments, the model of the worker-managed enterprise has emerged as a key element in new thinking about the transition to socialism based on the conceptualization of worker-management as a training ground for nurturing socialist relations and values.

Much of this fascination with industrial self-management can be traced to the rediscovery and popularization of the work of the great Italian Marxist theorist Antonio Gramsci, who, more than any other theorist of the left, viewed organization and struggles at the point of production as the energizing forces for breaking capitalist domination and catalyzing the process of revolutionary social change.[2] Perceiving the transition to socialism as a very long-term process dependent upon the readiness of the working class for its historic task, Gramsci places the struggle for democracy at the workplace at center stage as the great classroom for the self-education of the working class.

Gramsci's great theoretical contribution was to grapple, with some success, with the question of why socialist revolutions had not occurred in the developed capitalist countries where presumably all of the objective material conditions existed for such revolutions. Gramsci suggested that the continuation of capitalist domination might be explained by the existence of both coercive power, situated in the institutions of the state, and the *hegemony* of civil society, or domination of capital through the instruments of ideology, legitimation, and false consciousness. In the Western nations where the state is merely the outer "ramparts," it is hegemony (the spontaneous loyalty that dominant groups gain by virtue of their social and intellectual prestige and by their leadership of productive and political life, and conversely, the resignation and confusion that arises out of the experience of being dominated [Williams, 1960]), that must be opposed by a working-class counter-hegemony as a prerequisite to social transformation.

In his earliest works, in particular, Gramsci placed great reliance on worker-councils as the seed of a new working-class counter-hegemony. Impressed by the ubiquity of soviets in all revolutionary struggles and particularly impressed with their role in the October Revolution, Gramsci conceived of worker-councils as natural proletarian institutions in which workers might enter into socialist relationships and incorporate socialist values and activities. He argued that by organizing at the workplace, by taking over production from management, and by producing cooperatively, workers would be educated to socialism and gain the capacity to rule society. In these institutions of self-education, workers would acquire skills, capacities, responsibilities, and confidence in their abilities as a class. In these councils were the tools for enabling the formerly subordinate class to gain a belief in itself as a ruling class.[3]

Despite their many theoretical differences, these positions have in common the belief that even in the midst of a larger capitalist society, worker councils and other forms of cooperative, collective organization at the point of production might represent powerful schools for the self-education of the working class, that such institutions might provide a setting in which values and behavior appropriate only to wage-labor under capitalism would be dissolved and those more appropriate to socialism would be given birth. What is so striking about the presumed relationship between self-management and the nurturance of socialist orientations, however, is the almost absolute paucity of data that would enable any reasonable judgment to be made about its veracity. I address myself to that shortcoming by generating data that should enable us to begin to answer the question of what happens to people who work in self-managed enterprises, to ask what value and behavioral lessons are learned in them, and to ask how people emerge in a political sense from such experiences.

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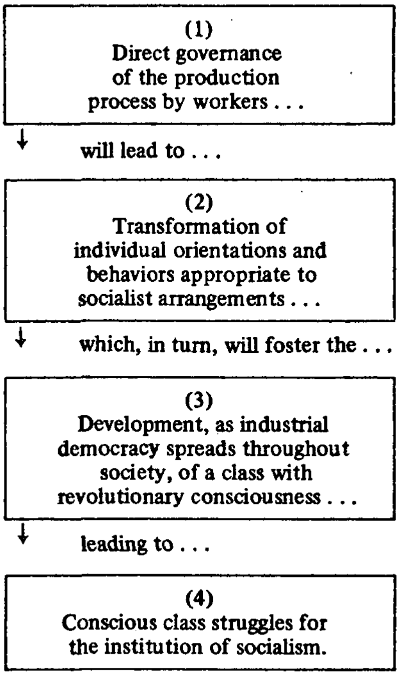
[2] Since the rediscovery and translation of Gramsci's works in recent years, there has been a virtual avalanche of Gramsci scholarship. Among the very best secondary works are Anderson (1976), Boggs (1976), Cammett (1967), Fiori (1970), and Williams (1975). For primary sources see Gramsci (1954, 1957, 1973).

[3] Gramsci's views on worker-councils differ dramatically from many other enthusiasts of self-management, particularly those in the United States, in two important respects. First, is his position that worker-councils are incapable of completing the revolutionary task by themselves. It is striking that from the earliest days of his euphoria about the prospects of the councils, through the collapse of the Turin Council movement, to the writing of his main theoretical work in Mussolini's prisons, Gramsci always linked the councils to the larger struggle for socialism and to the necessary efforts of a vanguard party as general educator and coordinator. Second, Gramsci conceived of worker-councils as spontaneous organizations that arise out of struggles at the point of production—as natural proletarian institutions which are not only tools of class struggle but also the effect of such struggle. Councils are not, in short, formal organizational divisions created by unions, management, or government. Nor are they producer-cooperatives, the organizations which form the basis of the present study. In more contemporary times, similar positions on councils and the larger revolutionary movement have been taken by Gorz (1967), and Mandel (1970, 1973).

It might be helpful to present the preceding theoretical claims in the following schematic representation:



(1)
Direct governance of the production process by workers . . .

will lead to . . .

(2)
Transformation of individual orientations and behaviors appropriate to socialist arrangements . . .

which, in turn, will foster the . . .

(3)
Development, as industrial democracy spreads throughout society, of a class with revolutionary consciousness . . .

leading to . . .

(4)
Conscious class struggles for the institution of socialism.

The limited purpose of this article is to examine the linkage between (1) and (2) by comparing the effects on the basic orientations of those employed in worker-managed enterprises with the orientation of people whose productive work is confined to conventional, hierarchical enterprises. What the article cannot do is to examine either the link between (2) and (3) or between (3) and (4), which require an entirely different form of scholarly investigation.[4] Taken as a whole, however, should no relationship be found to exist between worker self-management and the transformation of values and behavior (linkage 1→2), then the case for industrial democracy as a tool for radical social change must be seriously doubted.

The Research Population and Data Collection

In most advanced industrial systems, the internal organization of productive enterprise more closely approximates authoritarian political forms than democratic ones, a generalization that holds for both capitalist and socialist societies, the democratic rhetoric on both sides notwithstanding. The most notable exceptions to this state of affairs in the United States have been producer-cooperatives. As the only large-scale industrial enterprises in the United States where the people who work in them are in complete control, producer-cooperatives offer an unusual and timely set of examples for examining some of the effects of cooperative productive labor from which we might cautiously generalize. While such cooperatives are not necessarily models for all of industry, nor sites where the aforementioned theories of the political left can be directly tested, they can serve to highlight basic questions about the democratic governance of industrial working life and its various effects. Cooperatives are, in short, useful sites in which to observe some of the outcomes of collective decision making within an individualistic market society.

Our research sites are locted within a rather remarkable concentration of 14 worker-owned and worker-managed industrial enterprises in the Pacific-Northwest plywood industry. Although elaborate descriptions of these firms already exist (Bellas, 1972; Berman, 1967; Bernstein, 1974; Greenberg, 1979), I will introduce them briefly. Most of the worker-owned plywood firms, which account for over 10 percent of all plywood production and are generally conceded to be the source of the industry's highest quality product, were founded in the years immediately following the Second World War. Several were started from scratch by lumber workers worried about the dismal employment prospects in their industry, several were the product of workers seeking to prevent the closing of a privately owned mill, and more than a few were created by promotors who took a healthy slice of initial profits for their efforts. Without exception, studies of the plywood industry indicate that the worker-owned firms, which range in size from 80 to 350 members, are characterized by higher levels of productivity than are conventional firms.

The data for this study were generated over a two-year period in three ways. First, overall descriptions and assessments of the operations of

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This article examines the status of individual attitudes and not class consciousness per se. It is not appropriate to simply aggregate individual attitudes to understand the latter, for class consciousness is an attribute of a class not of individuals. More concretely, class consciousness involves the formation not only of shared attitudes but also of communication networks, distinctive institutions, and politiccal organization among structurally located population groups. For the outstanding historical works that sensitively examine the formation of class consciousness in this sense, see Thompson (1963) and Trotsky (1959).

producer cooperatives and conventional plywood plants were gathered by means of interviews I conducted with people familiar with both the industry and/or particular companies. Second, I concluded in-depth interviews over a four-month period with 22 worker-owners in three producer-cooperatives; with 5 hired workers in these same companies; and with 11 workers in a neighboring, conventional, non-producer-cooperative plywood company, all with an eye towards gaining a richly textured sense of the particulars of the work experience, the social dynamics internal to the plants, and the felt effects of such an experience. Finally, I collected data from 551 people (worker-owners in four cooperatives, hired workers in these same companies, and workers in a single large, conventional, unionized plant)[5] by means of a lengthy mail questionnaire.[6] All of the companies involved are in close geographical proximity, are identical in terms of the technical organization of production, and are practically indistinguishable in terms of the demographic characteristics of the work force. That being the case, these firms provide an unusually well-defined scientific sample in which enterprises vary primarily with respect to the central variable or set of variables—the nature of the participatory experience—but in no other important respects. Such a sample of companies serves to isolate the effects of participation in decision making by diminishing the possibility that observed variations between the companies might be explained by differences in industrial processes, work routines, geographically specific cultures, work-force demographics, and the like.

The Governance of the Plywood Cooperatives

I am concerned with the effects of self-management on orientations and behavior relevant to social change rather than with the self-management process itself, which has been extensively elaborated elsewhere. Nevertheless, it is worth noting that the cooperatives are, in fact, truly self-governing institutions and not what some would call partial or pseudo-participatory institutions (see Bellas, 1972; Berman, 1967; Bernstein, 1974; Greenberg, 1979).

In worker-owned firms, ultimate authority and responsibility on *all* policy matters are vested in the general membership of the cooperative, expressed concretely in the annual or semiannual general membership meeting. While the membership in practice may choose to delegate many of its responsibilities to the general manager and/or the Board of Directors, in a formal sense the purview of the shareholders' meeting is unlimited, responsible as it is for the total governance and direction of the enterprise. If they so choose, shareholders at these meetings may decide to fire the manager and hire a new one, alter hourly wage rates, build a new plant, or do whatever else seems appropriate to the general membership. In such membership meetings, each shareholder is entitled to a single vote even if, as in a few exceptional cases, a member owns more than one share.

At these meetings, a board of directors is elected from among the shareholders, a body for which all members are eligible. This board is charged with general policy making in the interim between general membership meetings. The board is not entirely free in policy matters, however, since all of the firms require the board to gain permission from the general membership for any expenditures beyond some specified maximum figure.

Other features common to the formal organization of the plywood cooperatives are worth noting. All, for instance, operate on the principle of equal hourly wage rates among the shareholders no matter the number of years on the job or level of skill. All divide the annual surplus (the amount that remains from total income after hourly wage rates, operating costs, taxes, investment, and savings have been deducted) to the shareholders *as workers* rather than as dividends on capital. That is to say, nonworking shareholders are not entitled to a portion of the surplus. All hire a management team (a general manager and assistants) that is responsible for the day-to-day operations of the firm, subject to the guidance of the board of directors (which usually meets biweekly) and the suffrance of the general membership meeting. All insist that potential worker-owners spend a probationary period working in the plant and that their membership be formally accepted by either the general membership meeting or the board of directors.

The democratic character of the formal organization of the plywood cooperatives is enriched by the democratic and participatory social interactions in the everyday operations of these enterprises. Interviews focusing on internal social relations show the existence, for instance, of a strong sense of collective/mutual responsibility among co-op members as compared to conventionally organized firms. Co-op workers consistently demonstrate a significantly greater tendency to cooperate informally on production problems, to rotate jobs, and to create innovative solutions to production bottlenecks.

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[5] The union (control) plant that was selected is characterized by excellent non-management relations and relatively high morale as indicated by the absence of strikes over the past decade and the very high average tenure of its workforce (equal to that of the cooperatives).

[6] The return rate on the mail questionnaire was a surprisingly high 63 percent. Preliminary analysis of data gathered during the summer of 1978 from a sample of the remaining 37 percent shows no systematic variation on major variables in comparison with those in the main data set.

Profound differences between plant settings are also evident in the workers' relationship to their supervisors. The cooperatives are easily able to manage production with no more than *two* foremen per shift, and often with only *one,* while conventional plants often require *six* or *seven.* Since there are significantly fewer of them in the co-ops, supervisors are not "on the backs" of the production workers, but are forced to be concerned with broader, plant-wide issues having to do with the flow of materials and machine use.

Besides the general looseness and distance of supervision, a state of affairs that allows a great deal of free space for individual initiative and informal cooperation between production workers, there is also a marked contrast between cooperative and conventional mills in the style of interaction between supervisor and worker. In the conventional plant, communication is hierarchical and one-way, with orders coming from the top, and compliance (whether willing or unwilling, enthusiastic or begrudging) coming from below. In the producer cooperatives, communication is two-way, open, and relatively free-wheeling, characteristic of communications between relative equals.

Enterprise-level policy making is also marked by democratic social relations. One is immediately struck in the cooperatives by the degree to which information is widely available to the worker-owners and the extent to which working shareholders are generally informed about and interested in enterprise policy. Also noteworthy is the level of discussion about enterprise concerns. About 65 percent report that they regularly discuss production, finance, sales, and investment policies with other working shareholders. The cooperatives are also characterized by high attendance at shareholder meetings (about 95 percent) and active involvement of their members. Thirty-one percent of shareholders report that they "very often" or "always" participate in the discussions at shareholder meetings.

As to the board of directors, an elected representative institution comprised solely of working shareholders and in many respects the key decision-making body in the cooperatives, we find a continuation of the democratic social relations found in other operations of these enterprises. For one thing, no single clique seems to run the board. All are characterized by significant membership turnover. An impressively high number of shareholders, moreover, have run for the board (31.5 percent), have been elected and have served (17.5 percent). Most importantly, however, the board seems to avoid the problem of distance from the constituency so characteristic of representative institutions because of the inescapable fact that board members continue to hold jobs in the mill during their tenure and are completely accessible to the shareholders.

The Effects of Self-Management

It can be said without much hesitation, then, that the producer cooperatives in plywood are genuinely self-managed enterprises, being institutions in which all policies are subject to the control of the work force. Having established their credentials as institutions that embody most of the features lauded by theorists of industrial democracy and self-management, we now turn our attention to delineating the effects of these arrangements on the orientations and activities of those involved in them, particularly as they relate to the possibilities for social change. The data will demonstrate that the hopes of many leftist theorists in the educative possibilities of self-management operating within a capitalist-market economy are *not* fulfilled. Indeed, the data unambiguously indicate experiences within such enterprises have precisely the opposite results, cementing and firming-up the orientations and behaviors most appropriate to life in a market society.

Joining a Producer Cooperative

Perhaps the first key to understanding the social and political meanings of the cooperatives is to note the selection process at work. Almost without exception, people join the plywood cooperatives for financial rather than political reasons. That is to say, both in the interviews and in the questionnaire responses, the number of people reporting that they joined because of the work environment, or the nature of the relationships with supervisors and other workers, or to participate in the processes of industrial self-government, is conspicuously low. The primary motivation for joining the producer-cooperatives in plywood is connected, in one way or another, to the desire for financial gain and job security. It should be remembered, of course, that the people who join such cooperatives are distinctly working-class in origin, many of whom have had periods of economic difficulty in their lives, and have experienced significant periods of unemployment—47 percent have been unemployed, with 46 percent of that total experiencing over 5 months of unemployment. The fact that once one becomes a member of a cooperative, one is guaranteed a job if the firm remains in operation is no small attraction to people who worry about financial insecurity. Furthermore, given workers' limited ability to save and declining opportunities in the small-business sector, the producer-cooperative becomes one of the few outlets that working-class people have for making a small investment. Jobs and investment are, thus, the recurrent themes in workers' explanations for joining the plywood cooperatives. These themes are evident in the figures below which represent the responses of shareholders when asked to judge the degree of importance of the following factors in joining a cooperative. Table 1 shows that financial investment is the strongest motivation for joining a cooperative, closely followed by the importance of good wages and job security; conversely, those reasons related to the internal governance and political processes of the cooperatives make a dismal showing.

These figures are strongly buttressed by interview responses from shareholders. Not a single respondent, spontaneously and without prompting, mentioned even vaguely political or ideological grounds for joining these enterprises. Almost all were quick to mention the attractions of financial investment and job security, a surprisingly large number mentioned the desire to escape labor union/company tensions and uncertainties, and not a few seemed to have accidentally wandered in for no particular reason. Political motivations were notable for their absence:

> Buying a job . . . it was a job. The plant's running 7 days a week, 12 hours a day ever since I've been here. So, if a guy wanted to really get in to work, he could make all the overtime he wanted. So he could actually bring home a pretty good check. . . .
>
> Well, I thought I wanted to buy into a small business . . . be my own boss. And my brother-in-law convinced me that would take too long and it would be more risky than [this].

What is implied in the above comments is that a rather special stratum of the blue-collar working class finds its way into the cooperatives, namely, those most oriented to the world of business and those financially able to make a down payment on a share. This hint that cooperative membership might be characterized by a small-business or petit-bourgeois[7] background and outlook is confirmed by several additional pieces of information. First, when compared to workers in a conventional plywood plant, shareholders are significantly more likely to have owned a small business in the past or to own one concurrently with their present work. Furthermore, this more pronounced small-business linkage exists among the newest members of cooperatives when compared to workers in the conventional plant (Gamma = .45 for those working in their present firms for four years or less), suggesting that those who *enter* into the former are a particular element of the working class. Another indicator of the small-business tendency in the cooperatives is the slight propensity for their membership to identify with the Republican party, traditionally the party of *small-business* ideology in the United States. To be sure, given the distance that most respondents feel themselves to be from the political system as a whole, the data on party identification, taken

Table 1. Reasons for Becoming a Shareholder in a Producer-Cooperative

Question: "How important were each of the following in your decision to *first* become a shareholder in this cooperative?"

	Percent Answering "*Very* Important"
Heard that workers in the plant got along well with each other	17.9
Heard that supervisors and workers got along well	17.1
Thought it would be a good financial investment	66.4
Wanted an opportunity to help run a company	12.5
Wanted a guaranteed job in case times got hard	42.1
The wages were good	48.2
There were no other jobs available at the time	15.0
Total = 280	

Source: Collected and compiled by the author.

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[7] In its pure form, the petite bourgeoisie is that class centered in the simple-commodity sector of the economy. In contrast to both the bourgeoisie and the proletariat, it own property, produces goods and services directly but does not hire and live off the labor of others. Politically it has traditionally been characterized by its support for a free market of small producers and its hostility to both labor unions and giant corporations. In this article, the term "petite bourgeoisie" will be used in this technical sense, not in the frequently encountered perjorative sense.

alone, are not overwhelming. Nevertheless, combined with the information on small-business ownership, it is suggestive of a form of small-business mentality among this section of the working class. This impression is given substance by the class identifications of the respective work forces. Among a sample that agreed to the proposition that social classes exist in the United States, cooperative shareholders were significantly more likely than workers in the conventional plant to conceive of themselves as middle-class, a tendency that is most pronounced among the newest members (Gamma = .35 for those working in their present firms for four years or less).

There is also some indication that the people who choose to join the plywood cooperatives are a more generally satisfied lot than others. On an open-ended question asking respondents what changes they would like to see made in the United States, among the newest members of each of the types of enterprises, shareholders were almost twice as likely to give totally supportive responses (over 20 percent of the sample) than were workers in conventional firms. Furthermore, among these newcomers, shareholders were significantly more likely to agree with the proposition (Gamma = .29) that "Our current economic system gives almost everyone a fair chance to get what they need to live a decent life."

The Effects of Industrial Self-Management

The cooperatives, relative to conventional firms, thus tend to attract members from what must be seen as a particular stratum of the blue-collar working class, that stratum characterized by a positive attraction to the market system and by a desire to leave the ranks of wage-earners and to enter the more privileged world of small property holders. Nevertheless, whatever workers' reasons for joining a producer cooperative, shared cooperative social relations at work might have unintended consequences not anticipated by those who join such enterprises. This is certainly the position taken by almost all of the theorists of industrial democracy, e.g., that the intentions of involved individuals are irrelevant. What matters for the development of particular attitudes among workers (whether they favor greater democratic participation or greater support for cooperative social relations) is the actual experience of cooperative labor.

In fact, the in-depth interviews with workers and observations at the plants give some indication that such unintended consequences might indeed result from working in the producer-cooperatives. It has already been pointed out, for instance, that shareholders are more likely, compared to workers in conventional companies, to enter into informal cooperative arrangements in activities that range from interacting in general shareholder meetings to mutually working out production problems that invariably arise during the course of building plywood panels. I have also discussed the impressive degree to which a tone of mutuality and group responsibility pervades these unique enterprises. Also worth noting is the existence of equal hourly wage rates within the cooperatives and the very strong, almost universal support for this form of compensation among shareholders. However, the overwhelming impression from the data that follows is that the experience of self-management within enterprises oriented to sales in a competitive marketplace is to enhance and nurture the small-property/petit-bourgeois orientations that shareholders bring with them upon joining. What this suggests is that the cooperative social relations that do exist are entered into and maintained for the pursuit of small-property ends. In this respect, those theorists of industrial democracy who see self-management as an important school for the development of orien-

Table 2. Percentage of Workers Having Owned a Business

Question: "Other than owning a share or a piece of stock in a company, have you ever owned a business of your own?"

	Shareholders in Cooperatives	Workers in Conventional Union Company
Owned a business previously but not now	17.6	7.0
Now own a business	6.8	2.3
Never owned a business	75.6	90.7
N =	250	129
Gamma =	.50	

Source: Collected and compiled by the author.

Table 3. Party Identification among Plywood Workers (Percent)

Question 1: "Generally speaking, do you usually think of yourself as a Republican, a Democrat, or an Independent?"

	Shareholders in Cooperatives	Workers in Conventional Union Company	
Republican	10.5	5.4	Gamma = .05
Democrat	47.8	54.3	
Independent	33.6	30.2	
Unsure/Other	8.1	10.1	
N =	247	129	

Question 2: "If you chose Independent, do you consider yourself as closer to the Republican or Democratic party?"

	Shareholders in Cooperatives	Workers in Conventional Union Company	
Republican	22.4	10.0	Gamma = .11
Democratic	38.8	52.5	
Neither	38.8	37.5	
N =	85	40	

Source: Collected and compiled by the author.

tations favorable to socialism would seem to be very wide of the mark, indeed.

Perhaps the most direct way to see this is to compare shareholders and workers in conventional companies on their support for classical liberal values, that set of values which arose with the appearance of market society. These values have been described as those of "possessive individualism," or "competitive individualism," and entail the familiar notions that each individual is responsible for his/her own fate in a perpetual competition for rewards against others in society, that success in the competition for rewards is justly arrived at through the marketplace, and that those who succeed owe nothing to those who fail in this competition, especially through the intervention of government. In order to tap this value dimension, I constructed an index of classical liberalism using responses to the following statements:[8]

Table 4. Class Identification among Plywood Workers (Percent)

Question: "If you agree that there are social classes in America, do you generally think of yourself as working-class, middle-class, or upper-class?"

	Shareholders in Cooperatives	Workers in Conventional Union Company
Working-class	54.5	66.7
Middle-class	45.5	33.3
Upper-class	0.0	0.0
N =	231	117
Gamma =	.25	

Source: Collected and compiled by the author.

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[8] A note on methodology is in order at this point about each of the indices which follows. Each is based on responses to items in the mail questionnaire. Each is a simple additive index based upon five response Likert-type items (from *strongly agree* through *strongly disagree*). Items were selected for each index by subjecting pools of potential items with evident surface similarity to factor analysis procedures (principal factor with iteration, varimax rotation). Once items falling along a unidimensional scale were identified, indices were constructed by summing the responses and recoding the

1. Many people are poor today because they are really not willing to work.
2. When people don't succeed financially, it's almost always their own fault and not society's.
3. A person should basically depend on himself and not ask or expect the help of others.
4. Most people wouldn't work if they didn't need the money for eating and living.
5. A person's first duty is to himself and his family.

Several things are immediately evident with regard to Table 5. First, both workers in co-ops and workers in the conventional firm strongly affirm classical liberal values, suggesting that support for competitive individualism may be so pervasive in American life that it even includes the great mass of the blue-collar working class. This finding tends to confirm those of a number of scholars (Lane, 1962; Sennett and Cobb, 1973; Terkel, 1974; Wills, 1970) and suggests the distance that must be traveled by those who perceive of the possibility of a political movement of the left in the United States in the near future. A second finding, most important for the immediate purposes of this article, is that shareholders in cooperatives tend to affirm classical liberal values more strongly than their counterparts in conventional work, though the relationship is weak.

This relationship is unaffected by controlling for time on the job. That is, the overall Gamma (.22) is not changed by introducing a control for job tenure (first-order Gamma = .22). The tendency for shareholders to hold more strongly to classical liberal values is evident throughout the entire range. Thus the overall (weak) relationship reported in Table 5 is given substance by its consistency throughout the entire range of work experience. That is to say, the experience of self-management (as it is practiced in the plywood cooperatives) does not, whether taken in isolation or as compared to work in conventional enterprises, serve to undermine or diminish the commitment to classical liberal values, as most theorists of industrial democracy have speculated. The persistence of the belief in a world characterized by self-interested struggles for survival in a group experiencing shared-cooperative productive relations must certainly give us pause.

These findings are given added emphasis by the results of an index designed to measure the belief in equality among workers. For many theorists of industrial democracy, one of the principal processes by which societal transformation is encouraged is through the generalization of beliefs in human equality gained through the cooperative-egalitarian setting of the self-managed enterprise to other spheres of life. The following measurement of egalitarian beliefs does not support the hypothesis, however. An index was created by summing the responses to the following questionnaire items:

1. A group of equals will work a lot better than a group with a strong boss.
2. Each person should get what he needs; . . . the things we produce as a society belong to all of us.

Table 5. Indices of the Sociopolitical Orientations of Plywood Workers (Percent)

	Shareholders in Cooperatives	Workers in Conventional Union Company	
1. Index of Classical Liberalism			
Lowest (5-12)	2.0	3.2	
Neutral (13-17)	30.6	40.0	Gamma = .22
Highest (18-22)	67.4	56.8	
2. Index of Egalitarianism			
Lowest (3-7)	26.3	20.2	
Neutral (8-10)	52.9	46.8	Gamma = .23
Highest (11-15	20.9	33.0	
3. Index of Support for the Welfare State			
Lowest (6-14)	38.5	15.2	
Neutral (15-21)	51.2	63.2	Gamma = .46
Highest (22-30)	10.3	21.6	

Source: Collected and compiled by the author.

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final scores into 5 categories corresponding to the Likert pattern. Comparisons were then run on each index contrasting respondents in the producer-cooperatives with those in the conventional plant.

3. For society to run better, the really talented people should be rewarded more than others.

The rather startling finding reported in Table 5 is that workers in conventional plants organized in traditional hierarchical ways are more likely to affirm general egalitarian values than shareholders in the plywood cooperatives where work is carried out under general conditions of equality. That is to say, the experience of egalitarian relationships in the cooperatives is not generalized by shareholders so as to incorporate the world beyond the walls of the enterprise. Again, while the strength of the relationship is not overwhelming, it is given added strength by its consistency with the results on the index of classical liberalism. The tendency to reject or to not affirm general egalitarian values is perfectly consistent with a belief in the reality and benefits of the competitive struggle. Furthermore, as with the previous index, the difference between the two groups holds throughout the entire range of job tenure (first-order Gamma = .25).

This finding that the effects of the self-management experience do not scale the walls of the enterprise to incorporate persons and institutions on the outside, that the general small-property outlooks that shareholders bring with them to the cooperative relative to the world around them are not diminished by virtue of their experience, is given further weight by responses to a question asking whether or not wages throughout American industry should be made as equal as possible. Recall that shareholders are themselves paid in an egalitarian fashion, and that to affirm this question would in some ways represent but a simple generalization of their own experience, one that has proved quite successful. Surprisingly, workers in a conventional firm proved somewhat more likely to affirm the more egalitarian position on wages than shareholders. (The relationship holds throughout the entire range of job tenure.) Again, while the relationship is not overwhelmingly powerful, its consistency with the previous indices on classical liberal and egalitarian values is noteworthy.

This general picture of a small-property/petit-bourgeois orientation among shareholders in the plywood cooperatives is given further weight by workers' responses to an index that seeks to measure reactions to the welfare state. While none of the particular items in the index measures reactions to public policies constitutive of socialism per se, the following questionnaire items all relate to public policies whereby government serves as a tool for egalitarian redistribution, for public control of the economy, and for the general health and welfare of the citizenry. It is a measure, if you will, of support for policies at the left wing of the modern liberal welfare state, and as such, for policies closely related to a more egalitarian/publicly controlled social order. The questions which made up the index are as follows:

1. Should the federal government own and operate key industries?
2. Should the federal government control how much profit a company can make in any one year?
3. Should the federal government pay for medical and hospital bills?
4. Should the federal government control the prices set by large corporations?
5. Should the federal government control big corporations so they would better serve the public interest?

The results, as reported in Table 5, are unambiguous: shareholders are much more likely to reject the public policies of the welfare state, to take the more conservative (classical-liberal), small-property position than are workers in the conventional industrial enterprise. Once again, these differences hold throughout the entire range of job tenure. The initial differences between worker-shareholders and workers in the conventional firm

Table 6. Workers' Opinions Regarding the Equalization of Industrial Wages and Salaries (Percent)

Question: "Some people have suggested that wages and salaries throughout American Industry be made as equal as possible. How do you feel about this suggestion?"

	Shareholders in Cooperatives	Workers in Conventional Union Company	
Agree and strongly agree	34.1	39.5	
Neither agree nor disagree	11.5	16.3	Gamma = .15
Disagree and strongly disagree	54.3	44.2	

Source: Collected and compiled by the author.

on their support for welfare state policies do not disappear over the years but are strongly maintained.

One of the hypotheses about the effects of industrial democracy advanced by both its friends and enemies is that the alteration of the traditional form of hierarchical work organization causes workers to question authority relationships in general. That is to say, according to the theory, under conditions of full self-management, workers not only come to see that hierarchical authority relationships at the point of production are superfluous, but come to generalize that view to the level of both the firm and society outside of the walls of the firm. To advocates of social change on the left, this possible social dynamic remains one of the most exciting possibilities of industrial democracy because it both dissolves the bonds to the status quo and forms the nucleus for a new kind of society. To defenders of the status quo, this dynamic forms the contours of a social nightmare (Crozier, 1975). We shall see that conservatives have little cause to worry.

In an attempt to tap the effects of the work experience on general orientations to authority, I constructed an index based on the following statements:

1. People in positions of authority are owed our highest respect.
2. Patriotism and loyalty are the most important requirements of a good citizen.
3. People should maintain their respect for the police even when the police are clearly in the wrong.

A comparison of shareholders in cooperatives and of workers in a conventional firm show almost no differences worth considering (Gamma = .03), a somewhat surprising finding in light of the non-hierarchical productive relationships found in the former. That is to say, the experience with authority relations is *not* transferred beyond the walls of the firm.[9]

Thus far, we have confined our attention to the possible effects of the experience of self-management on fairly abstract or general attitudes of the population of the producer-cooperatives. What we have found is that the cooperative work experience is either not sufficiently profound or relevant to certain domains (attitudes toward authority, government, the economy) to show any differences between worker-shareholders and workers in conventional settings, or tends to have effects opposite to those predicted by most advocates of industrial democracy (such as attitudes toward class identification, classical liberalism, egalitarianism, the welfare state). That is to say, with respect to general politico-social orientations, the experience of self-management in the producer-cooperatives has no effect in some cases on the general orientations typical of the blue-collar working class, and in others, seems to produce orientations even less conducive to societal change in an egalitarian/communitarian direction.

What is even more startling, however, are the findings that show no significant effects in relationship to social processes more directly derived from everyday experiences in the producer-cooperatives. According to many advocates of industrial democracy and social change, one of the most likely results of worker self-management is that workers should gain increased confidence in their fellows. One is supposed to find, in a kind of ascending dynamic, an initial confidence in those immediately around one spreading outward to eventually include the entire working class. According to this view, having seen that ordinary working people are capable of running all of the affairs of their own enterprises, workers gradually come to believe in the ability of ordinary working people to run the entire society.

The data in this study cast grave doubts on this theoretical dynamic. In order to test the effect of self-management on orientations to the blue-collar working class in general, I asked respondents to assess the general managerial capabilities of other working people. Despite the fact that worker-shareholders (who themselves are no better trained or more skilled than other working people) have been competently, skillfully, and profitably running their own industrial enterprises, this experience does not seem to increase their confidence (the relationship holds throughout the entire range of job tenure), relative to people in conventional companies, in the abilities of other working people to do the same thing in their own firms, although a slight plurality of shareholders is willing to affirm this proposition. That is, their own direct experience is not generalized to the blue-collar class taken in its entirety.

When we turn to a question that asks about confidence in the ability of working people to run

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[9] Given this finding that workers' direct experience with a different form of authority at work is not transferred to their general orientations toward authority outside of the factory walls, one should not be too surprised to learn that the experience of self-management in the producer-cooperative does not seem to affect orientations toward business (on indices of corporate power and between worker-shareholders and workers in the conventional firm shows no differences in orientations toward government (on indices of corporate power and corporate role in society) and the economy (indices on the efficiency and fairness of the economy). Limitations of space preclude detailed reports of these findings.

Table 7. Plywood Workers' Attitudes about the Role of Workers in Society (Percent)

	Shareholders in Cooperatives	Workers in Conventional Union Company	
1. Even without the help of managerial personnel, working people have enough skill and intelligence to run everyday production in their plants.			
Strongly agree	10.3	12.3	
Agree	34.5	33.8	
Neither agree nor disagree	13.9	16.2	Gamma = .07
Disagree	29.8	30.8	
Strongly disagree	11.5	6.9	
2. Even without the help of managerial personnel, working people have enough skill and intelligence to run the business and financial affairs of their plants.			
Strongly agree	2.0	2.4	
Agree	8.7	10.9	
Neither agree nor disagree	9.1	21.1	Gamma = .26
Disagree	55.7	50.0	
Strongly disagree	24.5	15.6	
3. Except for issues like wages and benefits, workers in conventional, privately owned plants should have little to say about running their plants. Those who own companies should run them.			
Strongly agree	13.5	10.9	
Agree	40.9	37.5	
Neither agree nor disagree	14.7	18.0	Gamma = .08
Disagree	25.8	28.1	
Strongly disagree	5.2	5.5	
N =	252	128	

Source: Collected and compiled by the author.

not merely the technical, production side of enterprises but also the business affiars of enterprises, activities even more distant from the direct experience of most working people, we find the above findings further strengthened (see Table 7). Not surprisingly, of course, most respondents tend to discount the abilities of their fellows. What is surprising, however, is the degree to which workers in conventional companies are slightly *more confident* in other members of the working class than are worker-shareholders who actually participate in the business affairs of their enterprises, enterprises which have proved quite successful in a business sense.

Finally, all of these findings about workers' orientations toward other blue-collar workers are further confirmed by a question that asks directly about the possibility of the dissemination of forms of industrial democracy in the American economy (see Table 7). Besides the interesting finding that neither group in our sample seems strongly supportive of industrial democracy, what is even more to the point is that experience in a self-managed enterprise does not enhance such support relative to other work experiences.

Conclusion

The expectation held by many theorists of industrial democracy that self-managed work environments might serve to nurture feelings of cooperation, equality, generosity, and self-confidence in one's fellows is only partly met within the plywood producer cooperatives. The expectation that such feelings would spill over the walls of the workplace so as to incorporate society, economy, and government is decidedly *not* met in the cases here under review. Indeed, the findings point to the opposite results. With respect to the outside world, the producer cooperatives seem to nurture outlooks characterized not by community, mutuality, equality and confidence in others, but outlooks more congruent with the tenets of classical liberalism: those of individualism, competition, limited government, equality of opportunity and inequality of condition, and so on. The data indicate that those entering the cooperatives bring with them a small-property/petit-bourgeois experience and outlook, and that tenure in the cooperatives serves both to maintain some elements of this orientation and to enhance others.[10] The cooperatives thus seem to take people with attitudes appropriate to a market economy and nurture them further in this regard. It would seem, then, at least with respect to the cases reviewed here, that self-managed enterprises operating within a system of market capitalism seem unlikely settings not only for the practice of socialist relationships but especially for the promulgation of general attitudes appropriate to a more egalitarian/cooperative society.

The producer cooperatives in plywood represent the most fully developed self-managed enterprises existing in the United States today. As such, they represent perhaps the best setting in which to observe the effects on workers' political and social outlooks of the experience of industrial democracy in enterprises operating within an overall market system. What the findings seem to suggest is that the *market* is a more powerful educative tool than is the cooperative experience itself. Or to put it differently, so long as the economic well-being of enterprise members depends upon success in the marketplace, whether it be based on ownership of shares, or on profit sharing, bonuses, or other such devices, behavior appropriate to that success will be encouraged even within self-managed enterprises.[11] This is especially true in settings where a political organization devoted to general coordination and socialist education (in the Gramscian sense) does not exist. One gets the strange result, then, of a collective/cooperative/egalitarian spirit that is somewhat encouraged within the enterprise walls running up against the harsh rocks and shoals of the marketplace. What this should further suggest to theorists of the left is that while industrial self-management is surely the model by which a decent and humane society will organize its productive life in the future, the worker-managed workplace may not be, isolated as it is, an appropriate educative setting in the present context for nurturing a larger political movement for change.

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[10] The argument that the outlook of shareholders is best characterized as a small-property/petit-bourgeois orientation is further affirmed by findings that indicate the existence of a strongly anti-corporation, anti-big government outlook among worker-shareholders. On an index designed to tap attitudes about large corporations among shareholders, less than 10 percent responded positively while well over 50 percent responded negatively. Other indices demonstrate strong distaste for government intervention and initiatives in economic and social affairs. The sample almost universally opposes current tax and spending levels. This complex combination of attitudes about individualism, competition, government, and concentrated economic power is the essence of classical liberalism, what we would today call conservatism.

[11] It might be argued that producer-cooperatives, because they are enterprises owned by workers, represent a special case and are therefore inappropriate for the examination of the theoretical issues raised here. To be sure, producer-cooperatives are by no stretch of the imagination worker-councils in the Gramsci, Mandel, and Gorz sense. Nevertheless, producer-cooperatives can give us an opening wedge in the understanding of some of the effects of cooperative labor in a distinctly hostile setting. Indeed, except for cases in which full-scale revolution is in progress, it is difficult to imagine enduring cases of self-management in industry which can in actuality or in theory escape the inherent logic of producer-cooperatives, e.g., collective control of enterprise productive apparatus with an eye towards profit making in the marketplace. What we have found to be the case in terms of attitude development in the producer-cooperatives, then, must surely hold to some degree in any self-management arrangement situated in capitalist market societies.

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Political Efficacy at Work: The Connection between More Autonomous Forms of Workplace Organization and a More Participatory Politics

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Carole Pateman (1970) argued that democratized industrial authority structures would require workers to develop precisely the skills and resources necessary for participation in political life beyond the workplace. She based her argument on a close analysis of classical democratic theory and the indirect empirical evidence available at the time concerning workplace-related political participation. Although a great deal of research in industrial psychology and sociology and organization behavior is available, none treats expressly political correlates of workplace democracy. Analysis of data from a new factory designed on the basis of semi-autonomous, self-managing work groups provides empirical evidence directly supporting Pateman. The analysis also results in empirically grounded scales that differentiate between work humanization *and* work democratization. *The latter correlates significantly with a sense of political efficacy. Further research to support democratizing industrial authority structures seems warranted for a political science that would contribute to a more participatory politics.*

Prior to Carole Pateman's *Participation and Democratic Theory* (1970), political scientists paid scant attention to workplace experience as a potential source of political learning (Almond and Verba, 1963; Lipsitz, 1964, being notable exceptions). Pateman carefully reviews classical democratic theorists (including J. S. Mill and G. D. H. Cole) and the substantial empirical literature on participation in organizational decision making (relying, in part, on Blumberg, 1968) to support her argument that industrial work resting on an undemocratic authority structure socializes people into passivity and political apathy. She suggests in her concluding chapter that the redesign of workplace authority structures following democratic principles would spill over beyond the workplace to reinforce greater participatory democracy instead of democratic elitism.

One of Pateman's major contributions is her definition of work organization as a possible determinant of political behavior. Hers is the first political science assessment of the quality of work life (QWL) in the context of democratic theory. However, despite her comprehensive assessment of the empirical evidence on participation in workplace decision making and evidence on the sense of political efficacy, she could not produce any direct empirical evidence linking the two. As Ambrecht notes (1975, p. 19), the "problem with Pateman's analysis is that she assumes away precisely that which needs to be investigated empirically: the link between participation in the workplace and participation in political life." The main purpose of this article is to report empirical research on this link.

Pateman's problem, of course, was that at the time of her analysis (the late 1960s) there was almost no empirical evidence concerning such a possible linkage, as she herself points out (1970, pp. 106-07):

> The major difficulty in a discussion of the empirical possibilities of democratizing industrial authority structures is that we do not have sufficient information on a participatory system . . . to test some of the arguments of the participatory theory of democracy satisfactorily.

This difficulty may be a result of the interdisciplinary nature of the problem. Political scientists and political sociologists interested in participation tend to define it in relation to the electoral system. They use occupational titles rather than looking inside organizations at the innards of QWL—concrete tasks and workplace relations—even when their subject is the relation between work and politics (e.g., Allardt, 1976). Conversely, psychologists, sociologists, and organizational researchers have produced an enormous body of research and theory about the inner dynamics of work organizations without paying the least attention to possible political ramifications outside the organization. Hence, another purpose here is to deepen and continue Pateman's definition of workplace political socialization as a political science problem.

Pateman's argument rests on a process of politically relevant adult social learning in the workplace. Participation is both the structure and the glue that holds the structure together. She sees the workplace as a potential training ground for acquiring participatory skills and resources, provided that it is organized in a participatory way. Work so organized would induce workers to develop the same skills, resources, and values that research on political efficacy has shown to be necessary for political participation in the larger society. Unfortunately, what Pateman means by a "participatory" work organization is not completely clear.

Pateman's analysis of classical democratic theorists shows "that we learn to participate by participating and that feelings of political efficacy are more likely to be developed in a participatory environment" (1970, p. 105). However, Pateman does not exactly define a "participatory environment" in the workplace. Students of organization and the quality of working life (QWL) have identified, studied, and manipulated work organization variables galore. Which of these contribute to a "participatory work environment"?

We cannot simply pull the necessary scales out of the hat of contemporary political science research. Nor can we uncritically take over scales and theories from the multitudinous literature in industrial organization, organizational psychology and sociology, etc., which is more concerned with productivity and job satisfaction than self-management and work democracy. Thus a third purpose of this article is to identify, both theoretically and empirically, indices that define a "participatory work environment" (what we call work democracy) and its politically relevant correlates.

We can summarize the argument in a simple model (Table 1). The critical issue is whether workplace experiences connect with political life. If empirical evidence linking Steps 2 and 3 is not forthcoming, Pateman's argument is seriously weakened. The link between Steps 1 and 2 is essential to establish that, in fact, a democratic work structure exists (Step 1) and has workplace consequences (Step 2).

Organization Design Concepts Necessary for Work Democracy

The first prerequisite of the model is obviously the design of a democratic form of work organization. Pateman's analysis of the participatory management literature failed to produce much empirical data based on work situations where workers actually made the final decisions concerning the management of their own work. But she found reason for optimism concerning future developments (1970, p. 105):

> The analysis of participation in the industrial context had made it clear that only a relatively minor modification of existing authority structures may be necessary for the development of a sense of political efficacy. It is quite conceivable, given recent theories of management, that partial participation at the lower level may become widespread in well-run enterprises in the future because of the multiplicity of advantages it appears to bring for efficiency and the capacity of the enterprise to adapt to changing circumstances.

Pateman's optimism seems to have been more than justified. The 1970s saw a virtual explosion of new theories of work organization under the generic name of QWL, which led to literally thousands of experiments in increased worker self-management. A comprehensive review is beyond my scope here, and several such reviews are already available. (In addition to the literature reviewed in Elden, 1975, see, for example, Cummings and Malloy, 1977; Cooper and Mumford, 1979; International Labor Office [ILO], 1979; and International Council for the Quality of Working Life [ICQWL], 1979.)

We will therefore only summarize some of the important concepts of organizational design as they have evolved over the last two decades. We limit our description primarily to work redesign experiments that aim at something more than merely a more efficient and flexible work organization. Pateman's democratized workplace clearly calls for something more than job enlargement

Table 1. A Simplified Model of Work Democracy and Its Organizational and Political Correlates

Step 1. Work Democracy →	*Step 2. Positive Quality of Work Life (QWL) Consequences* →	*Step 3. Political Consequences beyond the Workplace*
(production is organized around self-managing autonomous groups)	(self-managed workplace system results in increased levels of satisfaction, social appreciation, learning, etc., and becomes self-maintaining)	(politically relevant QWL factors connected with increased sense of political efficacy, politically relevant activity and better citizenship)

Source: Formulated by the author.

or participatory (i.e., "friendly") management. The key is a total redesign of the workplace so that workers control directly (instead of merely being consulted about) immediate workplace decision making.

In a democratic work organization, workers instead of managers make decisions about daily work schedules (who works when and where, doing what), personnel administration (hiring, training, rewarding, and disciplining) and those aspects of coordination most immediately affecting the flow of work for which the group is responsible (production management). This is something quite different from human relations training for supervisors. There is no place in a self-managed work group, for example, for the traditional role of a supervisor as "boss."

Organizational design principles to create precisely these democratic conditions in the workplace were first developed by researchers at the Tavistock Institute in London in the 1950s (see Herbst, 1962, 1976; Trist et al., 1973; and Emery, 1959, 1977; a more complete description is presented in Elden, 1976). They developed "sociotechnical" design principles that build on aggregated, "unit" tasks which include management and supervision functions and are performed by a self-managing or "autonomous" work group. The findings of the Tavistock researchers, originally derived from studies in coal mines, have been proven feasible under a wide variety of technological and organizational conditions. Even assembly lines in automobile manufacturing plants no longer seem inevitable as experiments at Volvo (Aguren et al., 1976), Volkswagen (Granel, 1979), and now General Motors (Guest, 1979) have demonstrated. These design principles have also been applied in the service of expressly democratizing change of the type postulated by Pateman. They were first applied in collaboration with Tavistock researchers in a series of government-sponsored shop-floor industrial democracy experiments in Norway in the mid-1960s (see Emery and Thorsrud, 1976, and Elden, 1979, for recent summaries). In the late 1960s and early 1970s they were applied in literally hundreds of experiments in Sweden alone, as well as in Europe and the United States.[1] Not all of this now-extensive literature reports on experiments that, like the Norwegian and a number of Swedish cases, were expressly designed to *democratize* working life. We return to the question of workplace democratization in constructing the scales necessary for an empirical assessment of Pateman's argument (operationalizing Steps 1 and 2 in Table 1).

We should make clear, however, that democracy at the workplace level—i.e., "work democracy"—is but a part of organizational or "industrial democracy," what Pateman terms "lower" —rather than "higher"—level participation. This distinction is quite common in the literature concerning the democratization of enterprises where direct (i.e., "participatory") democracy is distinguished from indirect (i.e., "representative") democracy. The former requires a completely new way of thinking about the design of work. The latter requires restructuring an organization's policy-making processes (e.g., worker councils, workers serving on boards of directors, worker ownership, etc.—see Elden, 1976, for a complete discussion). These two approaches are aimed at quite different problems, but, as Pateman argues (pp. 106-07), they should ideally be developed at the same time in the same company so as to reinforce each other. She also makes clear that the political socialization resulting from participation requires the redesign of work organization that affects everyone's everyday working life. Policy-level participation by a few elected representatives would not be sufficient.

In Table 1 we identified critical linkages in a simple three-step model. On the basis of our brief review of the QWL organizational design literature concerning self-managing work groups, we can now extend the model. The key, following Pateman's argument, is to move from hierarchical/elitist to self-managing/democratic authority structures so that social learning results in an increased sense of political efficacy rather than feelings of apathy and alienation. In Table 2 we elaborate the basic elements of our model and illustrate what kind of political learning we would hypothesize under the two different organization designs.

Even if we follow Pateman and build on the recent literature on work democracy experiments, we soon discover that virtually none of these explores the possible social learning effects of potential political relevance beyond the workplace. The study reported here is an exception. Our data were acquired in a new plant expressly designed according to QWL organization design principles (autonomous work groups). The express intention was to create a non-hierarchical authority structure where workers would make important decisions in managing their own work.

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[1] The Norwegians, followed by the Swedes, are generally considered to be pioneers in the theory and practice of work democracy. See Elden (1979) for a summary and analysis of the Norwegian experience. The Swedish Employers Confederation (1975) summarizes experiences from some 500 "experiments" while Stymne (1980) provides a more current and in-depth analysis of long-term government-sponsored work-democratization experiments in 5 companies. For collections of cases and studies of the field in Europe, see for example, Cooper and Mumford (1979); International Labor Office (ILO, 1979); ICQWL (1979); and Jenkins (1973, 1975). For America see, for example, Cummings and Malloy (1977); Davis and Cherns (1975); Davis and Taylor (1979); Hackman and Suttle (1977); and O'Toole (1973).

Data Sources and Methodology

As more fully described in Elden (1976), the plant in which our data were collected, located several hours' drive from a major metropolitan area on the west coast of the United States, was built around a highly automated (i.e., relatively capital-intensive) process for producing a paper-based consumer product. The automated machinery was arranged in 5 parallel lines, each managed by a group of approximately 15 workers on a three-shift basis 5 days a week. Thus at the time of this particular study, approximately 6 months after the factory started production, there were 225 employees.

This plant was one of several owned by the same parent company in different parts of the country. It was new and designed quite differently from the others. In the other plants there was a traditional work organization with supervisors, inspectors, and relatively highly skilled maintenance technicians. In the experimental plant, workers formed self-managing groups which took over managerial and specialist roles. Each group, in addition to controlling a set of production lines, contributed staff to the warehouse and to maintenance. The group met 30 minutes before

Table 2. Illustrations of the Three-Step Model under Two Types of Workplace Authority Structures

Elements in the Basic Steps of the Model	Authority Structure	
	Hierarchical/Elitist	Self-Managed/Democratic
Step 1: Design of Work Organization		
1.1 Basic building block	A single individual limited to a single job (i.e., "One-man, one job")	A group of individuals responsible for a variety of jobs (i.e., "group work")
1.2 Flow of control-relevant information	Vertical, formal, and external to workplace	Highly horizontal, informal and within the work group
1.3 Locus of control of work flow variances	"Higher authority"	The work group as a whole
1.4 Job design emphasis	–fragmentation –repetition –social isolation –low choice –short-term training	–integration –variety –team work –decision making –continual learning
1.5 Supervisor's role	Full time, "boss"-policeman	Part-time, "consultant"-teacher
Step 2: QWL Consequences		
2.1 Motivation	Calculative, instrumental (money, security, status)	Integrative, expressive (competence, learning, craftsmanship)
2.2 Orientation to work	Extrinsic, individual	Intrinsic, collective
2.3 Most likely social learning	Deference to and dependence on organizational supervisors, fatalism, passivity	Dependence on self and team mates, self-reliance, initiative
2.4 Support for self-management	Low and inconsistent	High and stable (self-maintaining)
Step 3: Political Consequences		
3.1 Sense of political efficacy	Low	High
3.2 Feeling of personal potency	High fate control	Low fate control
3.3 Social participation	Little and passive	Much and active
3.4 Political system implications	Democratic elitism	Participatory democracy

Source: Formulated by the author.

going on shift to decide issues like who would work where. Workers with extra experience and training in one area trained fellow workers through a system of job rotation managed by the team. In addition, teams set production goals and participated in plant-wide annual budget forecasting.

They did not, however, control *what* was produced, as marketing and finances were controlled by corporate headquarters. These was no union. Nevertheless, the plant management was clearly attempting to develop a more democratic form of work organization explicitly following concepts from socio-technical theory. (McWhinney, 1972, describes the application of these concepts in the plant.) During the first year of operation these concepts were partially realized. Because of technical difficulties, each team started with a temporary manager. However, by the time survey data were acquired, midway through the first year, a majority of work groups had only part-time managers who functioned as consultants, not bosses. In addition, four groups were operating entirely on their own. We have, in short, at least a partial test of work democracy and an opportunity to explore its possible politically relevant consequences.

As a member of an organization development team that helped to implement the design of the plant, I conducted several studies during the plant's first year, including action research, participant observation, semi-structured interviewing, and a questionnaire survey (see Elden, 1976). The data analyzed here come from the questionnaire which was completed on company time by 205 of approximately 225 workers (93 percent).

Since one of my purposes was to develop empirically based indices in the workplace, the bulk of the items in the questionnaire were drawn from standardized scales which seemed to have a certain "face value" relevance. Questions concerning such issues as power, control, authority, autonomy, participation and influence were obviously relevant. Since the design of this organization and Pateman's theory both called for collaborative and participatory modes of work organization, variables concerning peer leadership and group process also seemed relevant. Even within these categories, one finds numerous scales and countless items that could be relevant. Ultimately, of course, the choice of a particular scale was not crucial as the scale items were to be the basis of new, more practically relevant indices.

For the sake of simplicity, the questionnaire was based on pre-tested and standardized scale items developed by the University of Michigan Institute for Social Research in its inter-company longitudinal study. These items, building on the theoretical work of Likert, have undergone extensive validation testing and were used in assessing some 37 industrial organizations with more than 30,000 respondents (Bowers, 1973; Taylor and Bowers, 1972). This theoretical framework also appeared relevant becuase it included both group process and interpersonal relationships on the one hand, plus peer leadership and team functioning on the other. Examples of the items from these scales that were used to operationalize the workplace dimensions of the model include the following (see Elden, 1976, for the complete questionnaire):

1. Concerning peer leadership, to what extent do persons in your team
   —help you find ways to do your job better?
   —provide the help you need so that you can plan, organize and schedule work ahead of time?
   —offer each other new ideas for solving job-related problems?

2. Concerning team functioning and effectiveness, to what extent does your team
   —plan together and coordinate its efforts?
   —make good decisions and solve problems well?
   —share information about important events and situations?
   —really want to meet its objective successfully?

3. Concerning supportive group processes, to what extent are the persons in your team
   —friendly and easy to approach?
   —willing to listen to your problems?
   —encourage each other to give their best effort?

Step 2 in our simple model is based on correlates of a more democratized workplace that includes support for workplace self-management and personal growth, job satisfaction and participation beyond immediate workplace. Data on these issues were captured by questions directly about the support of worker self-management and various dimensions of job satisfaction based on the Michigan questionnaire (satisfaction with the team manager, with progress of the organization, with the opportunity for furthering one's career within the organization, with the opportunity for performing the actual day-to-day activities in the workplace, and the extent to which workers looked forward to coming to work each day). Specifically, two questions were asked about participation and influence:

> When decisions are being made in this plant, to what extent are the persons affected asked for their ideas?

> When the persons affected are asked for their ideas, to what extent are these ideas paid attention to?

Finally, the amount of influence over team decision making at different levels of the organization was assessed according to the four questions developed by Tannenbaum (1968) in his work on control graphs.

In the actual company where data were acquired, it turned out that other plants with a similar technology and product in the same corporation had used another questionnaire to assess QWL dimensions similar to these. Selected items from this questionnaire were also used, as they could possibly provide a basis of comparison to other plants with the same technology, but a more traditional form of organization. This comparison later proved problematic, but the items are included in the data analysis to provide a more differential basis for the indices we develop there. All of these items were fixed choice along a Likert 5-point continuum.

In general, operationalizing Steps 1 and 2 in our model presented the problem of having to select from among a wide variety of very sophisticated conceptual schemes. The problem was the reverse concerning the outcome variables in Step 3 of the model. This again reflects the relatively large amount of empirical research focused on behavior within organizations. Political scientists, and others, of course, have developed widely used measures of explicitly political behavior outside of organizations.

The measure most used by Pateman (see esp. 1970, Ch. 3) is the sense of political efficacy. This scale was also developed by political scientists at Michigan, but they were not concerned about work organization. It consists of four items (see Campbell et al., 1960, 1964) measuring attitudes toward government which have been found to be empirically associated with political participation (see, for example, Milbrath, 1965). An important part of the belief in one's political efficacy, of course, is a minimum degree of self-confidence or belief in one's ability to perform adequately (Milbrath, 1965, esp. pp. 56-60). It therefore also seemed reasonable to explore this dimension of personal potency which was operationalized by selected items from Rotter's fate-control scale (see Rotter, 1966; Rotter et al., 1972). These items are of particular interest in that they focus on the degree to which one feels powerless and controlled by fate or luck rather than by one's own will. Seeman (1967) uses this scale to measure that dimension of alienation expressed as a sense of powerlessness. Finally, a third way of empirically exploring the extra-workplace consequences of work democracy was developed in behavioral terms by asking the extent to which an individual allocates free time to various kinds of community service and leisure organizations. The hypothesis here, of course, is that personal potency and a sense of efficacy in the workplace should correlate with a more active social life and higher degree of participation outside the workplace.

It should be noted that more extensive data collection was planned on politically relevant behavior off the job, but because of objections from management, these questions could not be asked. At one point management's sensitivity concerning questions about behavior outside the workplace was reflected in the workers' responses. The final item in the questionnaire was an open-ended question concerning membership in voluntary organizations and was completed by just under half the respondents. The response rate on most other items was approximately 100 percent. Later studies within the plant, including both participant observation and action research, did provide data relevant to these off-the-job issues, but not in a form which can be analyzed here (see Elden, 1976).

In summary, the questionnaire was constructed around the three steps in our model, with the bulk of the items concerning the workplace. Of the total of 72 items, over half (39 items) concerned those QWL conditions theoretically necessary for work democracy (i.e., structural conditions for step 1 such as group autonomy, peer leadership, supportive group processes, opportunity to participate in plant decision making, etc.). Approximately a third (25 items) concerned hypothesized politically relevant consequences either within the plant (i.e., step 2—which included 12 QWL items on work orientation, job satisfaction, support for self-management, and personal growth) or beyond (i.e., Step 3—which included 13 items on political efficacy, personel potency and social participation). The remaining items covered social background (age, education, income, previous work experience, etc.). The complete questionnaire is available in Elden (1976).

Data Analysis

Data analysis occurred in two phases, both relying on the linkage analysis technique developed by McQuitty (1957). This technique gives results similar to factor analysis, but without the necessity of extra-statistical assumptions. It is particularly well suited to reveal the interconnectedness of variables in a correlation matrix. All correlations are simple product-moment coefficients. Those on which the analysis rests are all significant at least to the .05 level unless otherwise indicated.

The first task was to identify the extent to which questionnaire items concerning Steps 1 and 2 (i.e., all QWL items) in the model (Table 1) clustered together into meaningful indices. A correlation matrix was generated and subject to a linkage analysis according to the procedures developed by McQuitty. This analysis produced 11 QWL indices (numbered 1 through 11 in the appendix) which in several important respects departed from the original scales in which these items had been grouped (see Elden, 1976).

The next task was to examine how these 11 QWL indices related to the political variables from the model's Step 3. This task was approached by generating a second correlation matrix containing the inter-correlations of each QWL index with each of the other 10 QWL indices and the 3 political variables (sense of political efficacy, personal potency, and social participation). A McQuitty linkage of this matrix revealed two clusterings or dimensions which I have called Networks A and B (see Figure 1). These appear to represent two quite different dimensions of working life. Significantly, only those QWL indices in Network A are systematically and strongly related to political variables. Let us examine Network A more closely.

Network A QWL indices emphasize variables relating to power, autonomy, decision making, and egalitarian norms (recognition, fairness, participation) plus all work-related outcome variables: self-management support and work orientation. Significantly, the pattern of relationships closely corresponds to that predicted: we hypothesized that the experience of autonomy in work would support personal development and further involvement in the system of self-management. In effect, Network A operationalizes the three elements of the democratization proposition and demonstrates that work democracy links empirically with self-management at work and a sense of political efficacy, personal potency, and social participation beyond work. This network identifies *a political dimension of everyday work life.* The remaining QWL factors (Network B) concern primarily group process and interpersonal aspects of work. These apparently do not have first-order politically relevant linkages.

Network A is built around key concepts in the socio-technical design of work organization and the democratization of authority: the core of this network is the strong connection ($r = .65$) between work democracy (Index 1: "Equity and Recognition in Decision Making and Influence") and intrinsically rewarding work (Index 8: "Satisfaction with Self-Management Development Opportunities"). Clustering around the former, as predicted, are all aspects of power, control, and authority in work: group autonomy, plantwide influence, and democratic, egalitarian norms (fairness, recognition, and openness). That these factors correlate negatively with hierarchical control over the work group (Index 2) is a further indication that the indices measure a relatively self-managing form of work organization.

Around Index 8, as emphasized in socio-technical theory, are all indices of satisfaction, personal growth, and support for the system of self-management indicating an involvement in developmental opportunities. Democratization, in short, is connected with autonomy and learning.

Finally, and most significantly, the developmental cluster (Index 8) connects to participatory behavior beyond the workplace, apparently on the basis that day-to-day work relations under self-management relate to the sense of political efficacy ($r = .28$) which in turn connects with personal potency ($r = .24$). In short, a democratic authority structure covaries with support for self-management, which in turn covaries with more control over one's life in general. Work democracy links with "political potency."

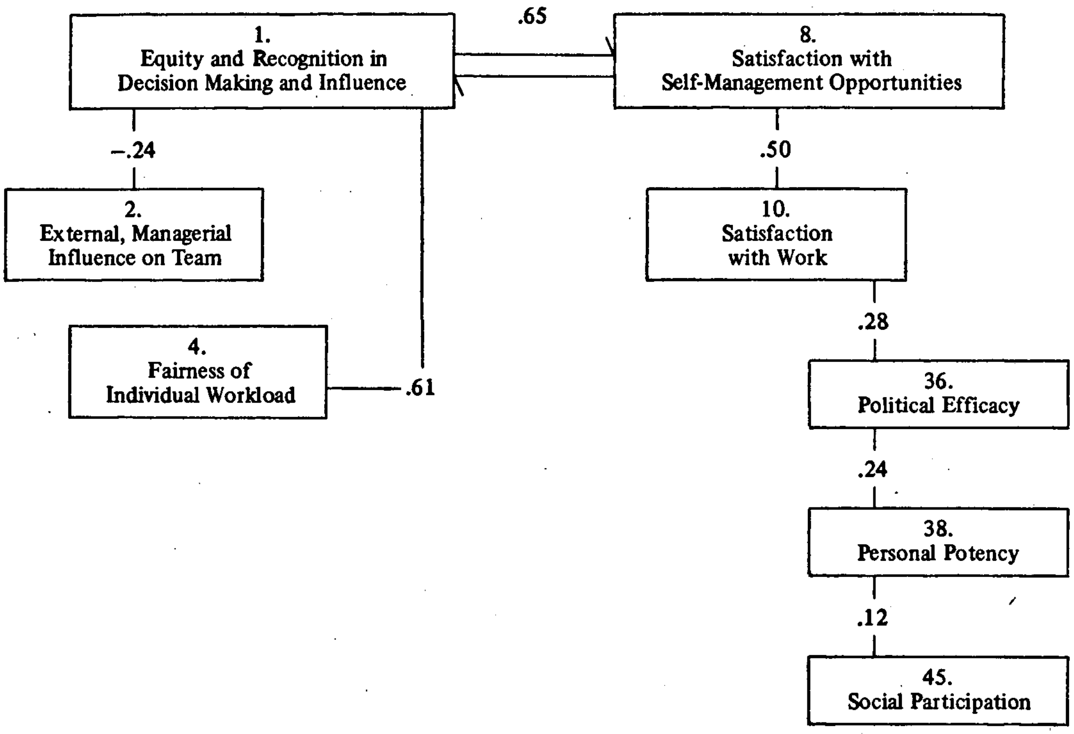
While Network A identifies the normative, power and authority features of work democracy and its politically relevant consequences, Network B focuses on its internal dynamics. Network B, identifying conditions for autonomous group competency, helps to operationalize another important dimension of the self-management design: its group basis. Consistent with socio-technical theory, Network B illustrates that team effectiveness and achievement (Indices 3 and 9) are a function of supportive interpersonal relations together with task mastery. Moreover, an effective autonomous group must possess management skills like planning, coordination, and cooperation.

In this plant, from the worker's viewpoint, effectiveness and achievement are not a function of an external, managerial structure but of the capacity of teams to manage themselves. Managerial influence (Index 2) is consistently and negatively related to all of the conditions for team competency, which reinforces the earlier argument that decreases in managerial authority have an impact on both the internal and external aspects of team autonomy.

Controls for age, education, income, and other background variables did not eliminate these correlations. Significantly, controlling for type of technology in the plant seemed to have more influence on the relation between work and the sense of political efficacy than did social attributes (see Elden, 1976).

The most significant finding is that the empirical data (i.e., Network A), are ordered precisely as hypothesized in our model. In other words, *having some power over one's work co-*

NETWORK A: Dynamics of the Self-Management System (Political Dimension of QWL).


1.
Equity and Recognition in Decision Making and Influence
.65
8.
Satisfaction with Self-Management Opportunities
−.24
2.
External, Managerial Influence on Team
.50
10.
Satisfaction with Work
4.
Fairness of Individual Workload
.61
.28
36.
Political Efficacy
.24
38.
Personal Potency
.12
45.
Social Participation

NETWORK B: Conditions for Team Competency (Non-Political Dimensions of QWL).

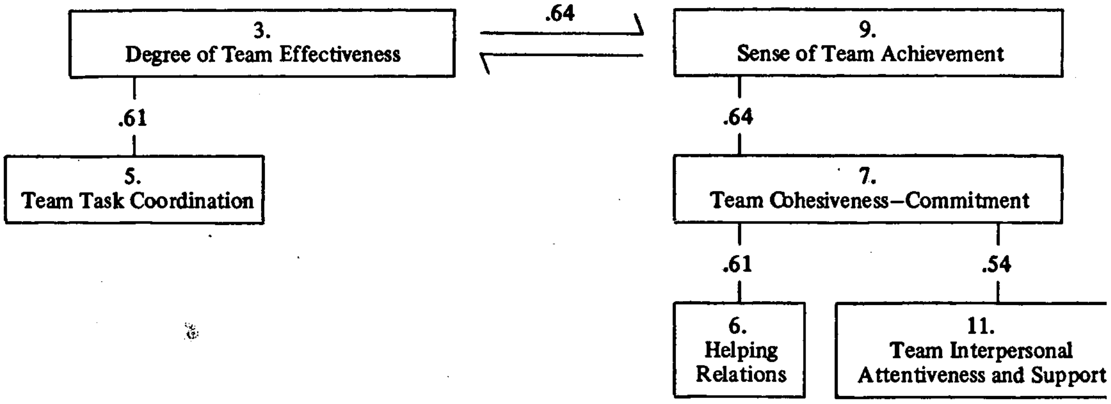

3.
Degree of Team Effectiveness
.64
9.
Sense of Team Achievement
.61
5.
Team Task Coordination
.64
7.
Team Cohesiveness–Commitment
.61
.54
6.
Helping Relations
11.
Team Interpersonal Attentiveness and Support

Figure 1. Dimensions of Politically Linked and Non-Politically Linked Quality of Working Life (QWL) Factors

Source: Formulated by the author.

varies with one's attitude toward taking up participatory opportunities. If there is no chance to have an impact, one does not attempt to participate: this is quite different from saying that personality, income, education or other individual psycho-social characteristics determine the degree of participation. These findings empirically support Pateman's contention that a democratic authority structure in a workplace is likely to enhance participation in and beyond it. Democratization at work is clearly linked to politically relevant variables beyond the organization. Empirically, there appears to be a *political dimension to QWL.*

A second significant finding, also in response to one of the main purposes of this research, is the identification of those QWL dimensions that have political relevance. Not surprisingly, these included all the items concerning control by workers over their own work and opportunities to influence other organizational decisions (Network A). Network B did not contain any items concerning power, control and authority. This conceptual clarification helps to make Pateman's "participatory environment" more operational. It also provides indices that can be used to guide organization redesign efforts more unambiguously in the direction of workplace democracy. At least conceptual tools for distinguishing between *work humanization* and *work democratization* are now available and it seems clear that a QWL strategy based exclusively on humanizing work is not as likely to foster democratic citizenship as one based on *democratizing* work.

Hence this research supports the importance of distinguishing between real and pseudo-participation that has been made on theoretical grounds (Pateman, 1970; Bråten, 1972) as well as on experimental grounds (Mulder, 1971). Simply enhancing the affective experience of workers—through sensitivity training, for example—would not be so likely to lead to an increased sense of political efficacy as would changes in task structures (especially power and authority relations), competencies and social norms which increased workers' relatively autonomous control over their work and influence in company affairs. Affective enhancement alone would imply a strategy of unobtrusively reinforcing hierarchical authority while developing a somewhat more satisfied, committed, and flexible work force.

It seems quite feasible to improve QWL by simply pleasing rather than empowering. Without empowering structural change, a QWL organizational development strategy like job enrichment or flexitime might lead to the reverse of work democracy. In the present case, analysis of participant-observation data (Elden, 1976) gathered after the survey and informal reports from the plant during the following several years indicate that management's long-run strategy may have been humanizing rather than democratizing. More contemporary efforts in other plants that are unionized suggest that union participation in work improvement programs may be a prerequisite for democratizing change (as well perhaps as authentically humanizing change)!

The findings of the present research are suggestive and possibly useful, but hardly conclusive. In the first place, the most critical correlation, between Steps 2 and 3, is only .28. While this is both theoretically and statistically significant, it is not impressive. Furthermore, it appears modest in comparison to those correlations between the intra-organizational QWL indices. There are several possible explanations for this. First, the scales from which the items composing our QWL indices were drawn have been tested and developed by many researchers over a long period of time. In contrast, politically relevant consequences might not be adequately tapped by the sense of political efficacy measure which was not designed for the purposes for which it is being used here. In retrospect, it seems likely that some of the most interesting and immediately relevant political consequenes were not captured by the two standardized scales allowed in the questionnaire. It might also be argued that 6 months was too brief a period in which to observe any connection between work-related political attitudes and political attitudes outside the workplace. A great deal more than 6 months were used to develop these attitudes in the first place. In short, this social learning process and the connection between work democracy and

Table 3. Evidence of a Political Dimension of QWL

1. Work Democracy →	*2. Positive QWL Consequences* →	*3. Participatory Politics*
Indices of autonomy, influence, and democratic norms (Index 1)	Indices of satisfaction, personal growth, and self-management QWL (Indices 8 and 10)	Indices of a more participatory political orientation (sense of political efficacy–Index 36)
$r = .65$	$r = .28$	

Source: Formulated by the author.

political learning constitute a more subtle relationship that is tapped by correlational data from standardized attitudinal questionnaires. Indeed, a later participant-observation study indicated many other connections (Elden, 1976). We suggest this issue as a subject warranting further research.

In summary, the basic pattern in the empirical data is quite clear: all correlations are in the hypothesized direction. All save one (social participation for which the data are inadequate) are statistically significant. More important, all are theoretically significant. The empirical evidence is patterned exactly as one would expect if participatory workplace structures did *in fact* facilitate politically relevant learning.

Discussion

Correlational data, of course, do not permit one to draw such a conclusion. Logically, causation can go either way. Workers with a high sense of political efficacy prior to beginning work in this factory may have responded more favorably to the opportunity for self-management provided by the new organization's design than workers without such a sense of efficacy. Causation is difficult to assess in data correlating attitudes with attitudes. Little of the relevant research that has become available since this study was undertaken (shortly after the publication of Pateman's thesis in 1970 [see Table 4]) avoids any limitations of the present study. All of these studies support my findings without extending them much. The research presented in Table 4 is limited to empirical studies in the 1970s grounded in some form of democratic workplace experience and its politically relevant correlates. Other bodies of literature such as those dealing with work and family, work and mental orientations (instrumental-intrinsic and alienative versus self-realizing), and work and society are excluded here because of space limitations and because they are less directly rooted in a democratic work organization. These are taken up in Elden (1976).

Unfortunately, there are few studies directly relevant to my subject and those that have been identified are limited to a particular democratic aspect of work such as autonomy, discretion, or self-direction. Nevertheless, the findings reported are consistent with the findings of this study.

The most striking supportive evidence comes from the first report of a series of studies by Karasek (1978), which explicitly focused on the social and political effects of work design ("job socialization") in a nationally representative sample of employed males in Sweden between the ages of 18 and 60, interviewed in 1968 and again in 1974 (N = 1638). Interestingly enough, as we have noted earlier, since 1969 Sweden has perhaps experimented more than any other country with democratic job reforms. While it is not yet realistic to evaluate discrete factory-level changes in Sweden on the basis of national aggregate data, Karasek shows that increases in *"job decision freedom" and "job demands" between 1968 and 1974 are associated with behavioral increases in political participation,* both among those who are highly active (e.g., those speaking at meetings, writing articles, attempting to influence others, airing complaints) and among those who are more than passive members of organizations (e.g., those participating in political or labor union activity, demonstrations). Karasek's measures of work democracy and its consequences are more demanding and comprehensive than those used in the present study. He has therefore a more solid basis from which to generalize and is able to make more useful distinctions.

Although we cannot fully deal with these here, Karasek's summary of the empirical finding makes a strong case for the political effects of work experience (1978, p. i):

> A test of the model with a longitudinal random sample of the Swedish work force (including both expert and self-reports of job characteristics) shows that workers whose jobs become more "passive" also become "passive" in their political and leisure activity. Workers with more "active" jobs become more "active" outside of work. Cross-sectional analysis yields consistent results. The alternative explanation that personality background characteristics account for the findings is undermined by the fact that the above associations change little when controlled for education and father's education. Further controls for family income, and kinship ties define boundaries to the job socialization effect, but do not substantially weaken it.

As is evident in Table 4, similar findings from quite independent studies in different countries about the expressly political effects of work democracy are paralleled by findings that work democracy also correlates with more active leisure time. Karasek, for example, consistent with our findings that work democracy covaries with personal autonomy and social participation, also finds that greater challenge and self-determination in the workplace closely correlates with a more active social life. This finding is not new (see, for example, Kornhauser, 1965; Blauner, 1964), but has been extended and supported by over a dozen studies since 1970, each using different methods with different kinds of workers in different countries (see Elden, 1976; Gardell, 1976; Emery and Phillips, 1976; Karasek, 1978, for comprehensive analysis). In short, the more

Table 4. Studies of Workplace Political Correlates (1970-1980)

Reference	Data Sources and Methods	Indices of a Democratic Workplace	Political Correlates				
			Political Efficacy, Participation, Information, etc.	Democratic Character and Citizenship	Trade Union and Other "Higher-Level" Participation Related to Work Interests	Active Leisure –Participation in Voluntary Organizations	Psychological Functioning, Mental Health, and Quality of Life
Martin Meissner (1971). "The Long Arm of the Job: A Study of Work and Leisure." *Industrial Relations* 10: 239-60.	Observation and interviews with 206 industrial workers in one company	Task discretion and social interaction			X	X	
Stanley Parker (1972). *The Future of Work and Leisure.* London: Paladin.	200 interviews and 425 questionnaires from respondents in selected occupations	Use of abilities in work and job autonomy				X	
Harold Sheppard and Neal Herrick (1972). *Where Have All the Robots Gone?* New York: Free Press.	Interviews with 371 industrial workers in 5 companies	Job anatomy variety and responsibility	X	X			
William C. Torbet and Malcolm Rogers (1972). *Being for the Most Part Puppets.* Cambridge, Mass.: Schenkman.	Observation and interviews with 209 industrial workers in 5 companies	Control over work and demand on skill	X	X		X	
Melvin Kohn and Carmi Schooler (1973). "Occupational Experience and Psychological Functioning: An Assessment of Reciprocal Effects." *American Sociological Review* 48: 951-61.	Interviews with sample of 3,101 male, civilian workers	Occupational self-direction		(X)		X	X

(continued)

Table 4. (continued)

			Political Correlates				
Reference	Data Sources and Methods	Indices of a Democratic Workplace	Political Efficacy, Participation, Information, etc.	Democratic Character and Citizenship	Trade Union and Other "Higher-Level" Participation Related to Work Interests	Active Leisure –Participation in Voluntary Organizations	Psychological Functioning, Mental Health, and Quality of Life
Gardell, Bertil (1976). *Arbetsinnehåll och Livskvalitet [The Content of Work and the Quality of Life]*. Stockholm: Prisma.	Interviews with 1500 workers in selected industries	Degree of competence and freedom in the work			X	X	X
Gunnela Westlander (1976). *Arbete och Livssituation [Work and Life Situation].* Stockholm: The Rationalization Council, LO-SAF.	3 studies, various methods, data from 2,387 industrial workers	Self-determination over one's work			(X)	X	X
Fred E. Emery and Chris Phillips (1976). *Living at Work.* Canberra; Australian Government Publishing Service.	Interviews with representative sample of 2000 urban workers	Autonomy, learning, and challenge				X	X
Winfried Hacker (1978). "Psychological Analysis and Design of Work Activities." *Nordisk Psykologi* 2: 114-26.	Reviews selected studies from Eastern Europe	Responsibility and freedom to decide tasks			X	X	
Robert Karasek (1978). "Job Socialization: A Longitudinal Study of Work, Political and Leisure Activity." Revised Working Paper No. 59. Stockholm: Institute for Social Research.	Interviews with a panel (longitudinal from 1968 and 1974) of 1638 workers representative of Sweden's total work force	Job discretion and engagement	X			X	

the political dimension of work develops toward democracy, the more productive use is made of free time and the more the characteristics of democratic citizenship are developed.

In short, the results of the research study reported here and all other empirical evidence which has become available since Pateman (1970) validate her contention that a democratically designed work environment induces the development of the type of political resources necessary for participation in and beyond the workplace. As Karasek observes:

> A telling finding in the Swedish data is that for workers with little education, the job is the primary classroom for transmitting patterns of leisure and political participation (Pateman, 1970). This job-centered educational process may only be successful to the extent that the work environment deemphasizes "passive" work that is emotionally undemanding and intellectually restrictive, and encourages "active" tasks involving psychological challenge and use of judgemental capacity (Karasek, 1978, p. 26; for a similar analysis, see the work of Ambrecht, 1975, 1976; and Inkeles, 1969).

With the exception of Karasek's research, the studies presented in Table 4 do not contribute much solid evidence on the issues raised by Pateman. These studies are difficult to compare, especially since none uses exactly the same measures of work democracy or of its politically relevant consequences. In addition, only 3 studies take up expressly *political* behavior. Furthermore, they are based on data from selected factories at one point in time and these data are only in some cases subject to correlational analysis. In short, research on this topic generally does not seem to have advanced much beyond the assessment by Seeman (1975, p. 108):

> What is at issue is the extent of generalization of the worker's experience, from the jobs proper to the extra-job milieu. Given the tendency to assume that work is of primary importance in defining the self and in connecting the individual with the larger society, and given the centrality of work in the Marxian sources of alienation, it seems surprising how limited the demonstration has actually been concerning this purported generalization.

This is not to say that research findings are in conflict. On the contrary, the findings of what little research is available exhibit a degree of agreement uncommon in the social sciences (Gardell, 1976). The main problem is that there has been too little research. One of the rare assessments of work organization from a political science point of view reaches exactly this conclusion and pinpoints the need for further research on adult political socialization in the context of work organization. As Ambrecht writes:

> Our review of the literature dealing with different cases of industrial democracy reveals that by and large, the political socialization effects accruing to increased workers participation within the work sphere have not so far been sufficiently explored. However, in those few cases where extraorganizational effects have been the object of inquiry, findings have unanimously pointed to politicization consequences ensuing from the democratization of the work organization. The redesigned work environment in which worker participation and control in decision-making is increassed, may thus represent a major means whereby lower status individuals may overcome socialized limitations and become full participants.
>
> Efforts at democratizing the world of work—which no doubt will become more numerous in the future—thus represent a fertile and largely uncharted research territory where adult political socialization processes can fruitfully be explored (1975, pp. 22-23).

Ambrecht focuses on the central research issue in Pateman's proposition: the "educative effects" of participation. A number of investigators have reported anecdotal evidence illustrating politically relevant social learning as part of QWL improvement programs. This needs to be more systematically explored.

Research here would help overcome the limitations of correlational data from a single point in time. It would naturally require a longitudinal panel or field study type of research design that focused on the process of politically relevant social learning. Data on structural and attitudinal change would be necessary not only before and after the experience of democratization but also *during*. The main variables could be drawn from those identified in Table 2 and the indices resulting from the present study (see appendix). An important task in such a research project would be to develop more sophisticated indices of politically relevant consequences outside the workplace.

Regardless of whether this particular design is used, the work democracy indices developed in this study could possibly be used by students of political efficacy and political participation with only a relatively minor extension of the scope of the research. Use of these indices, of course, is not as important as political science analysis of work organization and job design from the point of view of adult political socialization. If the design of democratic organizational structure provides an alternative route to full democratic citizenship, alternative to those of income and formal education, then political scientists need to begin looking inside organizations at how jobs are designed and how more democratic work experiences can be developed.

Work democracy seems to be one way of creating an enduring propensity to participate in political affairs. Learning to participate can be intrinsically rewarding so that political participation, once initially experienced, would be self-reinforcing. The key seems to be developing structures, such as work democracy or community councils or free schools, where people have participation, rather than apathy, reinforced by the organization of day-to-day life. Learning to participate in such structures appears to be highly likely. Research facilitating their development is essential for a political science that would contribute to a more participatory politics.

Appendix: Contents of the Quality of Working Life (QWL) Indices Used in Figure 1

I relied on a variety of standardized scales to gather data on QWL aspects of work democracy, but since one purpose of this research was to develop indices more expressly and empirically related to work democracy, the data analysis was based on the actual items in the original scales. As I have described more fully elsewhere (Elden, 1976), items are grouped together to form an index because linkage analysis revealed them to be more closely correlated with each other than with other items. The items composing each index are presented here to provide a more complete understanding of the different indices composing Networks A and B (Figure 1). The numbers of the indices below correspond to the numbers of the indices in Figure 1. Network A contains indices 1, 2, 4, 8, and 10. Network B contains indices 3, 5, 6, 7, 9, and 11.

Index 1: Equity and recognition in decision making and influence.

- —We feel we are treated fairly.
- —We feel our efforts are recognized.
- —We feel we are rewarded fairly for what we do.
- —When the persons affected are asked for their ideas, to what extent are these ideas paid attention to?
- —We have good ways of knowing how good our results are.
- —People are open and honest in talking with each other.
- —When decisions are made in this plant, to what extent are the persons affected asked for their ideas?
- —How much influence do team members have on what goes on in your team?
- —We help new people fit into the group.

Index 2: External, managerial influence on team activities.

- —The degree of top management's influence on what's going on in your team.
- —The degree of middle management's influence on what's going on in your team.
- —The degree of team manager's influence on what's going on in your team.

These items all derive from the Tannenbaum control graph scale (1968). Interestingly enough, the fourth item on this scale, concerning the degree of influence that team members have over their own team, is more closely correlated with Index 1. All three of these items correlate negatively with Index 1, indicating some empirical verification of a degree of team self-management or autonomy.

Index 3: Degree of team problem-solving effectiveness.

- —People in my group work well as a team.
- —We work effectively together to solve problems.
- —We are appropriately involved in decisions affecting our own work.
- —Team mates offer each other new ideas for solving job-related problems.

Index 4: Fairness of individual workload.

- —The amount of work I am asked to do is about right.
- —The pressure I work under is reasonable.
- —We get a fair shake when we do something wrong.
- —The things I need to be able to do my work right are available.

Index 5: Team-task coordination.

- —My team plans together and coordinates its efforts.
- —My team makes good decisions and solves problems well.

Index 6: Helping relations.

- —Team mates help me find ways to do my job better.
- —Team mates help me plan, organize and schedule work ahead of time.

Index 7: Team cohesiveness, commitment.

- —People on my team emphasize a team goal.
- —People on my team encourage each other to work as a team.
- —People on my team exchange opinions and ideas.
- —My team is able to respond to unusual work demands placed on it.
- —I have trust and confidence in the people on my team.
- —People on my team encourage each other to give their best effort.
- —My team really wants to meet its objectives successfully.
- —Information about important events and situations is shared within my team.

Index 8: Satisfaction with self-management development opportunities.

- —I support team self-management in this plant.
- —I am satisfied with the progress that I have made in this organization up to now.
- —I am satisfied with what I feel are my chances for getting ahead in this organization in the future.
- —All in all, I am satisfied with my team manager.
- —I feel that my pay is related to helping the company be successful.
- —There is an opportunity here for people to grow.

Index 9: Sense of achievement.

- —Morale in our team is high.
- —We really feel we are getting things done.
- —I know how well I am measuring up to my work.
- —People on my team maintain high standards of performance.
- —People on my team are friendly and easy to approach.

Index 10: Work satisfaction.

- —I enjoy performing the actual day to day activities that make up my functions.
- —I look forward to coming to work each day.

Index 11: Team interpersonal attentiveness and support.

- —To what extent are persons on your team willing to listen to your problems?
- —When you talk with persons on your team, to what extent do they pay attention to what you are saying?

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Language and Its Abuses in Hobbes' Political Philosophy

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As in Hobbes' view it is principally the capacity for speech that distinguishes men from even the social animals, so it is in verbal and doctrinal controversies that he usually finds the sources of conflict and sedition. Hobbes analyzes—in the hope of doing away with them—a variety of what he regards as abuses of language, such as metaphor, equivocation, eloquence, and absurdity, which are especially productive of political disorder. He also offers models and, in his own political philosophy, examples of the proper uses of language as science and counsel, which he believes are necessary to the establishment and governance of well-ordered commonwealths in the modern world, characterized as it is by widespread learning and disputatious habits. In the pursuance of his project, however, Hobbes himself is paradoxically forced to resort to the eloquence which he otherwise condemns, and his own observations on language provide grounds for doubts about the success of his enterprise.

Language and Hobbes' Analysis of Conflict

In all three of his political treatises (*Elements* I.19.5; *De Cive* V.5; *Leviathan*, Ch. 17, p. 111) Thomas Hobbes takes issue with Aristotle's comparison of political man to other gregarious animals, advancing six arguments to refute the suggestion that man is by nature a political animal like the bees or ants. His arguments are as follows: (1) Men, unlike bees, compete for precedence and honor, and for the "acknowledgment of wisdom" which can be a source of honor, and thus fall into hatred and disputes. (2) Man's private good, which lies in being eminent in "dominion, superiority, and private wealth," is not identical with the common good. (3) Men, having high opinions of their own wisdom, claim to perceive defects in governments and devise schemes for altering them. (4) Men, unlike bees, possess the "art of words" by which things can be made to appear better or worse than they actually are, and hence are able to "instigate one another to faction." (5) Men alone can quarrel over differing conceptions of right and wrong, and distinguish between "injury" and mere "damage." (6) And finally, the union of men is artificial, while that of bees is natural—an assertion whose demonstration constitutes the project of Hobbes' political theory as a whole.[1]

Four of the six specifically human attributes that Hobbes regards as conducive of political discord have to do with the faculty of language, a fact indicative of Hobbes' pervasive concern with words and doctrines as sources of conflict and sedition. "The tongue of man is a trumpet of war and sedition," he says; and the "elegant speeches" of Pericles confounded all Greece (*De Cive* V.5). Nor is it only in the spoken word, nor only in antiquity, that Hobbes perceives disturbing consequences in unrestrained speech. Speeches of republican Romans such as Cato, to the effect that "*all kings are to be reckoned amongst ravenous beasts*" (*De Cive*, Ep. Ded.), preserved and studied by young men at English universities, were in his opinion a principal cause of the Civil War in which he found his political teachings confirmed.

I shall argue, contrary to a common view,[2] that it is in speech in general, and in the misuse of words, that Hobbes usually seeks and finds the proximate causes of political discord. This is true in the state of nature, which, it has aptly been pointed out, is characterized by an "anarchy of meanings" as well as the absence of sovereign power (Wolin, 1961, p. 257), and it is likewise true in the imperfectly constituted commonwealths whose experience comprises most of history. This argument may be related to a larger theme which I shall not elaborate, to the effect that, in default of natural order and ends such as were posited by traditional natural law, Hobbes conceives the political problem as one of reconstituting the state as artifice, by human will (Oakeshott, 1975, p. 7).

I am grateful to Professor Michael L. Walzer, in whose Hobbes seminar at Harvard the ideas in this paper were first developed.

[1] Hobbes (deliberately?) misreads Aristotle, who argues (*Politics* 1253a1-18) that it is precisely speech that differentiates men from bees. Politics for Aristotle is to a large degree contestation in words over doctrines of justice. Cf. Weinberger (1975, p. 1339): "For Hobbes . . . opinion is inherently controversial."

[2] For example, McNeilly (1968, pp. 159-68) and Gauthier (1969, pp. 14-18) offer expositions of Hobbes' analysis of conflict without mentioning language abuse or verbal and doctrinal controversy.

It may simply be noted that this project depends in large degree on the correct application of language, from the form of words in the founding covenant to the sovereign's authority to promulgate right doctrine. It is also a precarious one, threatened at every stage by failure to reach agreement or abide by the rules established. And while Hobbes points to the persistence of private opinion (grounded in private passions) as the ultimate source of conflict, verbal disputes are its most common immediate and manifest cause.[3]

Despite his extreme individualism and materialistic motivation, Hobbesian man is not so much acquisitive as he is avid for power, honor, and preeminence.[4] Less widely recognized is the degree to which Hobbes presents the manipulation of words as an important, perhaps even the primary, means to both of these ends. Men achieve power by persuading others to follow them or their cause; men achieve eminence through the reputation of cleverness or wisdom. In either case, disputes arise over differences of opinion, which in the last analysis may often be reduced to differences over the meaning of words. Hobbes goes so far as to say that "all controversies" stem from contradictory use of such appellations as *meum/tuum,* just/unjust, good/evil (*De Cive* VI.9; cf. Watkins, 1968, p. 151). It is perhaps not always so simple; but Hobbes has no doubt, in his own age and country, of the danger not only of pernicious doctrines, but also of the emotive overtones and political impact of even a single word:

> From the reading, I say, of such books, men have undertaken to kill their kings, because the Greek and Latin writers, in their books, and discourses of policy, make it lawful, and laudable for any man to do so; provided, before he do it, he call him tyrant. For they say not *regicide,* that is killing a king, but *tyrannicide,* that is killing a tyrant is lawful (*Leviathan,* Ch. 29, p. 214).

Semantic disputes such as these in politics may have figured as prominently in motivating Hobbes' elaborate theory of language as did his campaign against the Aristotelianism of the universities in favor of the new science of motion (cf. Peters, 1967, p. 112). These two motives, however, are by no means unrelated. Absurdity, or the uttering of meaningless propositions, is a charge Hobbes frequently makes against both his political and his philosophical opponents; and absurdity he regards as a common pitfall of the use of words, which leads men to madness if not to wisdom (*Leviathan,* Ch. 4, p. 22), or indeed of thought, which for Hobbes is nothing more than the manipulation of words (*De Homine* X.1). But the political implications of language and its abuses go far beyond the dangers of absurdity, and Hobbes' theory of language advances from the unmasking of tautologies in scholastic Aristotelianism to the exploration of a variety of types of misuses of words for political purposes. Writing in an age of severe ideological conflict, Hobbes was the first modern political philosopher to become aware of the force of words and doctrines as political weapons, both exacerbating conflicts of interest and generating new conflicts of principle with a momentum of their own.

Differences of opinion, which prevent cooperation or a natural union of men, seem inevitable: men's passions and inclinations are diverse, stemming from their different "constitutions," and "good and evil are names given to things to signify the inclination or aversion of them, by whom they were given" (*De Cive* III.31). Differences of opinion, moreover, quite apart from competition over material goods, produce conflicts as a consequence of certain psychological mechanisms connected with the pleasure attached to honor:

> Again, when every man follows his own opinion, it is necessary that the controversies which rise among them, will become innumerable and indeterminable; whence there will breed among men, who by their own natural inclinations do account all dissension an affront, first hatred, then brawls and wars. . . (*De Cive* XVII.27).

In these circumstances Hobbes concludes that unrestrained liberty of opinion is incompatible with civil peace, and that "in the well-governing of opinions consisteth the well-governing of men's actions" (*Leviathan,* Ch. 18, p. 116). The third function of the sovereign, after wielding the "sword of justice" and the "sword of war," is "to make some common rules for all men, and to declare them publicly, by which every man may know what may be called his, what another's, what just, what unjust, what honest, what dishonest, what good, what evil. . ." (*De Cive* VI.9).[5] The authoritative definition of all such politically sensitive or disputed terms is thus claimed as a prerogative of the sovereign in the state (cf. Wolin, 1961, pp. 254-56, 259-60). Indeed, before the establishment of sovereign authority there cannot even be said to have existed any common sense of such words, whose *only* public meaning, Hobbes says, "proceeds from the right of the magistrate" (*De Cive* XII.1). It is not, furthermore, only words usually regarded as having political significance which must receive authoritative definition, but any term—even, ultimately, a term in a scientific dispute—that may breed controversy detrimental to peace: the city even defines what is a *man,* if "a woman bring forth a child of unwonted shape, and the law forbid to kill a man. . ." (*De Cive* XVII.12).[6]

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[3] Cf. Mansfield (1971, p. 100) on the Hobbesian covenant as a "bypass of [private] political opinion." Krook (1956, p. 20) says that "wrong linguistic usage is the *defining mark* of sedition."

[4] This view persists in opposition to Macpherson (1962). A historical version is presented by Thomas (1965), but the case rests on Hobbes' theory apart from his or its social affinities. The place of "vanity" in the motivation of Hobbesian men is stressed most notably by Strauss (1952, pp. 10-13).

[5] Parry (1967), Bell (1969), and Watkins (1968, p. 153) discuss ways in which Hobbes anticipates modern linguistic philosophy, particularly with reference to Austin's account of "performatives."

But while all public meanings are conventional, there is clearly a class of words of special political importance, confusion or disagreement about which is particularly likely to generate disputes. Hobbes points out that people make an invidious distinction, for example, between "aristocracy" and "oligarchy"—"which two words signify the same thing, together with the divers passions of those which use them" (*Elements* II.1.3; cf. *De Cive* VII.2). The common lawyers persist in Aristotle's obscure and question-begging definition of "justice" as "giving to every man his own" (*Dialogue,* p. 8). Sir Edward Coke, citing Bracton, insists that "law" is a "just statute, commanding those things which are honest, and forbidding the contrary," failing to understand the meaning of the word "command" and maintaining an illusory separate standard of justice (*Dialogue,* p. 25).

If the sovereign must in the end define such troublesome terms as these, political philosophy also has a role to play, counseling the sovereign when one exists, teaching true principles on its own account in times of disturbance, unmasking fallacies and abuses of words and reasonings at all times. "In the right definition of names lies the first use of speech; which is the acquisition of science" (*Leviathan,* Ch. 8, p. 22); and from correct definitions proceeds correct political science, and thence, peace in the commonwealth. I shall turn later to Hobbes' conception of science as one of the proper uses of language; first, however, I shall examine his criticisms of its various improper uses and the dangers thereby arising in political life.

Abuses of Language and Speech

Hobbes enumerates various abuses of language, among which the following may be mentioned. People can "devise errors and pass them on," errors that may arise from deceptions of sense, from words of inconstant or equivocal signification, from "words of things inconceivable," or from fallacious reasoning. They can speak passionately, or lie, or use language metaphorically, wresting words from their true signification "either to adorn or deceive," and using words as weapons "to grieve one another." And people may speak merely from habit, without thinking, or through reliance on unexamined authority (*De Homine* X.3; *Elements* I.5.14; *De Cive* XVIII.4; *Leviathan,* Ch. 4, p. 19).

For the most part these abuses appear to be perversions of legitimate functions of language. The principal such function is rational thought itself, including both science, or the formation and communication of true or hypothetical propositions, and the prudential or probable knowledge that is derived from reflection on experience. Although Hobbes claims that all reasoning is in the end at the service of the passions (*Leviathan,* Ch. 8, p. 46), he nevertheless maintains the possibility of exact and demonstrable science (geometry being the prototype) and of dispassionate, precise reasoning on any subject given prior agreement on first principles. Corresponding to this use of language is a class of abuses that may be called errors or fallacies: these are more characteristic of the written than the spoken word, and the pernicious influence of dubious philosophical doctrines in religion and politics stands out as a major example of this kind of perversion.

Hobbes also recognizes that speech is legitimately used to express certain passions, such as appetites (as in interrogation, requesting, prayer) and will (as in promising, threatening, and commanding). Speech functions such as these, however, may easily be transformed into such perverted forms as instigation and appeasement (serving to increase or diminish passion in another) or persuasion and exhortation beyond the limits of rational discourse (*Elements* I.13.6-7; cf. also *Leviathan,* Ch. 6, p. 38). Here we encounter a second class of abuses which in Hobbes' terms may be called passionate speech, perhaps qualified as speech immediately in the service of passions, and especially of passions other than fear or prudence.[7] For the political scientist the principal danger of this sort of abuse lies in the spoken word, with eloquence appearing as a frequent villain, although Hobbes has much to say, particularly with respect to the Civil War, about how written doctrines too can stimulate passions that find an outlet in violent speeches and sedition.

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[6] Compare the current controversy regarding abortion.

[7] Strauss (1953, p. 201; 1952, p. 150) argues that, although all reason is in the service of passion, Hobbes can maintain a notion of "right reason" as reason in the service of the greatest passion, fear.

Besides these two general classes of language abuses I shall consider two Hobbesian accounts of the motivation of such abuses, one a set of psychological observations about people's propensities to be ensnared by nonrational language, and the other a rudimentary theory of ideology, or the use of elaborate doctrine as a political weapon in the service of partisan interests or a will to power.

Turning first to Hobbes' theory of the passions and language abuse, we must recall that for Hobbes every word is a name that signifies a conception in the mind of the speaker. Conceptions, however, depend on objects as construed by the individual and are thus subject to the influence of his particular constitution and set of passions, which, because they may obstruct right reasoning, Hobbes calls "perturbations of the mind" (*De Homine* XII.1):

> And therefore in reasoning a man must take heed of words; which besides the signification of what we imagine of their nature, have a signification also of the nature, disposition, and interest of the speaker; such as are the names of the virtues and vices; for one man calleth *wisdom*, what another calleth *fear*. . . . Such names can never be true grounds of any ratiocination (*Leviathan*, Ch. 4, pp. 24-25).

The first problem of passionate speech thus pertains to names of "inconstant signification," whose referents are inescapably private and affective, at least in part. What kinds of words fall into this category? Hobbes asserts that all words (besides "is") fall into three classes: names of things, names of accidents, and names of names (common names for classes of things sharing certain accidents).[8] It is difficult to imagine passion intruding into the naming of bodies or of physical accidents, although errors of sense may create disagreements. Problems mainly arise with what may be called moral accidents, and it is generally moral epithets such as "good/bad" and "right/wrong" that Hobbes suggests are in need of authoritative definition. This list may be extended to include the names of the passions themselves, each of which according to Hobbes has two names, as "glory," depending on one's feelings, is equivalent to "pride" (*Elements* I.9.1); and to those names of names that depend on judgments about moral accidents, such as the contraries "democracy/anarchy," "aristocracy/oligarchy," and "monarchy/tyranny," which, Aristotle notwithstanding, signify not different things but conceptions differing according to the speaker's passions (*De Cive* VII.2). Without explicitly stipulated definitions no such words can be used for ratiocination, and even then their lack of tangible referents renders them always liable to inconstancy, or shifts of meaning under the influence of passions.

The problem of inconstant signification concerns a generally unconscious phenomenon. More easily identifiable, but perhaps more sinister, is the problem of equivocation—and of metaphor, since "every *metaphor* is by profession equivocal" (*De Corpore* II.12). Hobbes argues that names can be univocal or equivocal, that is, they may have precise or variable meanings, depending on how they are used, "for some use them properly and accurately for the finding out of truth; others draw them from their proper sense, for ornament or deceit" (*De Corpore* II.12). This suggests that by equivocation Hobbes refers to variability of signification that is normally conscious, even intentional. Any word can be made equivocal (*Elements* I.5.7), a fact that Hobbes attributes to the human faculty of quickness of mind, a peculiar verbal ability producing "those grateful similes, metaphors, and other tropes, by which both poets and orators have it in their power to make things please or displease. . ." (*Elements* I.10.4). Metaphor can of course be employed simply for ornamentation, although even in this case it can be dangerous, as in the Bible, which Hobbes insists must receive authoritative interpretation precisely because it contains so many metaphors: it is important for Hobbes' theology to realize, for example, that "spirit" and "inspiration" are used metaphorically, while "kingdom of God" is not (*Leviathan,* Chs. 34, 35, pp. 257, 264, 269). But equivocation also has its deliberate political uses, and in this regard also Hobbes has his suspicions about the Bible: he finds, for instance, that the word "justice" occurs frequently in the Greek and Latin versions, but that in English this is often subversively rendered "righteousness," "which few understand to signify the same, but take it rather for rightness of opinion" (*Behemoth,* pp. 242-43).

Beyond such particular difficulties lies the more general problem of elaborate speech, which is usually based on equivocation, and its manifestations as eloquence. Again, Hobbes is concerned with a certain type of fine speech, that in which "a metaphorical use of words [is] fitted to the passions," and of which the end is not truth, but victory (*De Cive* XII.12). Hobbes treats the problem of eloquence primarily in connection with the spoken word (rhetoric),[9] which in assemblies of men invariably becomes a weapon in the omnipresent competition for eminence (*De Cive* X.11). His criticisms of collective counsel as susceptible of kinds of oratorical imprudence to which the councillors separately would not be subject (*Leviathan*, Ch. 5, p. 30), and of democracy as a contentious "aristocracy of orators" (*Elements* II.2.5), are based on his observations of the readiness with which public speaking departs from the proper rational function of words:

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[8] On Hobbes' nominalism and its stark implications for ethics and politics, see Krook (1956), Goldsmith (1966, pp. 63-64), and Watkins (1965, pp. 255-57; and 1968, pp. 104-07, 144-50).

> Impudence in democratical assemblies does almost all that is done; it is the goddess of rhetoric, and carries proof with it. For what ordinary man will not, from so great boldness of affirmation, conclude there is great probability in the thing affirmed? (*Behemoth*, p. 250).

The lesson that Hobbes drew from Thucydides, too, was that reckless and dangerous policies are likely to result from the eloquence of demagogues who compete to flatter the vanity of their popular audiences:

> For a man that reasoneth with himself, will not be ashamed to admit of timorous suggestions in his business, that he may the stronglier provide; but in public deliberations before a multitude, fear (which for the most part adviseth well, though it execute not so) seldom or never showeth itself or is admitted ("Thucydides," p. xvi).

In his analysis of the civil disorder in England, Hobbes points to several areas where eloquence of the spoken word was a contributory cause. His attack on the common law. and his preference for clearly defined statute law, rests in part on the prevalence of equivocation in pleadings; this arises, in turn, both from the vagueness of common law terminology and from its admission of the legitimacy of the lawyer's interested position in suits, which induces him to try to "wrest the sense of words from their true meaning," and to cultivate a "faculty of *rhetoric* to seduce the jury, and sometimes the judge also, . . ." (*Dialogue*, pp. 6-7).

Political controversies feed on the disputatious habits of lawyers, whose profession teaches them "the art of cavilling against the words of a statute" (*Dialogue*, p. 45). They also arise, generally, from the liberty of public speaking on the part of private men, without license from the state, an invitation to sedition that Hobbes believes is unique to Christendom: "The heathen kings foresaw, that a few such orators would be able to make a great sedition" (*Behemoth*, p. 183). Hobbes deplores the disorders generated by the preaching of the itinerant friars of an earlier age, and he compares their role to that of the Puritan preachers of his own day, whom he describes in terms of their histrionics, their ability to feign inspiration, and their knowledge of how, in speaking, to play on the fears and interests of their audience (*Behemoth*, pp. 193-96). The proceedings of Parliament, finally, stand out as a case in point of the dangers of semantic abuse in a deliberative assembly, whether it be a council (as Hobbes regards Parliament) or a democratic assembly (as he accused it of regarding itself). Hobbes points to the meaninglessness of the terms used in the attainder of Strafford: he is accused of acting "traitoriously" (without reference to any statutory definition of treason), of subverting the "fundamental laws" (which are undefined), and of attempting to introduce "arbitrary government" (when in fact *all* government is necesssarily arbitrary)—and, in general,

> This [the Long] Parliament, in the use of their words, when they accused any man, never regarded the signification of them but the weight they had to aggravate the accusation to the ignorant multitude, which think all faults heinous that are expressed in heinous terms, if they hate the person accused (*Behemoth*, p. 249).

But eloquence is a hazard not only of spoken language, nor are Parliament and the law courts the only institutions that Hobbes finds guilty of uncritical speech. Written doctrine presents its dangers as well, and the universities with their Aristotelianism most often loom as Hobbes' favorite objects of attack. If public speech presents obvious dangers to peace, Hobbes also acknowledges that circumstances (such as "time, place, countenance, gesture," and so forth) may serve to clarify the meaning and intentions of the speaker (*De Cive* XVII.18). But written doctrine lacks these aids, with the result that, unless it is as precise as Euclid, it is perhaps even more susceptible of abuse than speech—though perhaps here it is often error rather than, or along with, the passions, which is to blame: "For there is scarce anything so clearly written, that when the cause thereof is forgotten, may not be wrested by an ignorant grammarian, or a cavilling logician, to the injury, oppression, or perhaps destruction of an honest man" (*Dialogue*, p. 64). Although it is possible to identify two types of such abuse, the disinterested errors of the "ignorant" and the directed equivocations of the "caviller," it is in practice not always easy to distinguish them, and the damage they do is similar. Academic disputings, moreover, are self-propelling: unclear writings elicit commentaries; the "commentaries will need explications; and in process of time, those explications expositions; those expositions new commentaries, without any end" (*De Cive* XVII.18). An end can be reached only through authoritative decision: Hobbes concludes that the sovereign must act as the final interpreter of Scripture and, by extension, of any scholastic dispute of consequence.

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[9] Strauss (1952) emphasizes Hobbes' debt to Aristotle's *Rhetoric*; the argument pertains, however, to Hobbes' doctrine of the passions rather than to his analysis of verbal conflict.

The cavillers and commentators to whom Hobbes refers are the philosophers of the universities, those whom he calls *dogmatici* in contrast to the *mathematici* of sounder method. The former put forward their opinions with passion (*Elements* I.13.4), like the adherents of the various Greek philosophical schools, of whom Hobbes says,

> Their moral philosophy is but a description of their own passions; . . . they make the rules of *good* and *bad*, by their own *liking* and *disliking:* by which means, in so great diversity of taste, there is nothing generally agreed on; but everyone doth, as far as he dares, whatsoever seemeth good in his own eyes, to the subversion of the Commonwealth (*Leviathan*, Ch. 46, p. 438).

More moderately, however, he also says that the *dogmatici* err in starting from unexamined custom or literary authority, "take the habitual discourse of the tongue for ratiocination," and proceed without "any evident demonstration." Both of these charges run through Hobbes' polemic against philosophers, although it is the latter, that of bad reasoning and absurdity, that is the more devastating. There is, in addition, the philosophers' characteristic urge towards novelty for its own sake: "In a multitude of men there are many who, supposing themselves wiser than others, endeavor to innovate, and divers innovators innovate divers ways; which is mere distraction and civil war" (*De Cive* V.5).

In good faith or bad, philosophy as traditionally practiced is in Hobbes' eyes a vast breeding ground of controversies, and in particular of the two major kinds of controversy from which England was suffering. In the first place, he charges that the hopeless incomprehensibility and contentiousness of Roman Catholic doctrine is due to its assimilation of Aristotelian teachings of the sort perpetuated at the universities; philosophy serves as a salve for "a great many absurd articles . . . which articles they thought fit to have believed, because they bring, some of them profit, and others reverence to the clergy, even to the meanest of them" (*Behemoth*, p. 215). Three of the four causes of "spiritual darkness" are doctrines maintained through scholasticism: erroneous interpretation of Scripture, the admixture with religion of erroneous philosophy, and "false, feigned, or uncertain" history (*Leviathan*, Ch. 44, p. 398). Cardinal Bellarmine's subtle distinction between temporal and spiritual authority "is but words" (*Leviathan*, Ch. 42, p. 378); and in general all theological controversies involve "words some of them without meaning, and nothing but the canting of Grecian sophisters" (*Elements* II.6.9).

In the second place it is the universities which, through the same philosophy, promote subversive political doctrines: "Studying Greek and Latin, [men] became acquainted with the democratical principles of Aristotle and Cicero, and from the love of their eloquence fell in love with their politics, and that more and more, till it grew into the rebellion (*Behemoth*, p. 218). It is these philosophers—"Plato, Aristotle, Cicero, Seneca, Plutarch, and the rest of the maintainers of the Greek and Roman anarchies" (*De Cive* XII.3)—who are guilty of making the meaningless verbal distinction between "kings" and "tyrants" which Hobbes is so at pains to expose (*Leviathan*, Chs. 29, 46, pp. 214, 447). Nor is it merely erroneous doctrine, but equally insidiously an overrated sense of their own wisdom, that the universities instill into their students, conducing to the dangerous habit of political criticism:

> For it is a hard matter for men, who do all think highly of their own wits, when they have also acquired the learning of the university, to be persuaded that they want any ability requisite for the government of a commonwealth, especially having read the glorious histories and the sententious politics of the ancient popular governments of the Greeks and the Romans. . . (*Behemoth*, pp. 192-93).

These attacks on erroneous or absurd philosophical doctrines bring us to the final category of language abuses, that pertaining to the use of "words insignificant" and words for "feigned things"—words which, although uttered, do not signify any coherent conception in the mind, and words which, though significant, do not refer to any "something" in the world (*De Corpore* II.6). In the second category are words which signify fictional things, clear mental conceptions arrived at through the faculty of "compound imagination," but which do not correspond to any real object: we have a meaningful name "centaur," or we can compound an image of ourself with that of Hercules or Alexander, "which happeneth often to them that are much taken with reading of romances" (*Leviathan*, Ch. 2, p. 10). Perhaps such political words as "democracy," under whose influence Hobbes saw his seditious contemporaries acting, are significant only of conceptions imaginatively generated through the association of a variety of images.

In contrast to these are words that are strictly insignificant or nonsensical, lacking even a coherent mental conception, but that are nevertheless used to the detriment of civil peace. A contradiction such as "round quadrangle" falls into this category of "senseless speech" that Hobbes calls absurdity (*Leviathan,* Chs. 4, 5, pp. 24, 27); so does the logical error of treating an abstraction (that is, a class of accidents) as if it were a body, as is done when one says (as Roman Catholics do) that "faith" is "infused."[10] Hobbes' favorite example of a senseless word that has had momentous consequences is "ghost"—which "signifieth nothing, neither in heaven nor earth, but the imaginary inhabitants of men's brain" (*Leviathan,* Ch. 34, p. 259). Ghosts, like the gods and demons of the Greeks, are the externalization of certain kinds of "phantasms" in the mind, of the sort that may be caused by a blow to the eye, refracted light, visual error, or day-dreaming; the conceptions behind them, expressed by the absurd phrase "incorporeal substance," are contradictory and empty.[11] But ignorance of how to distinguish dreams from reality on the part of the "simple people," who "are content to believe any absurdity, rather than to trouble themselves to examine it," is easily exploited by "crafty ambitious persons"; hence the belief in ghosts that produced gentile demonology was exploited by the Roman Catholic priesthood to justify their ritual practice (*Leviathan,* Chs. 2, 45, pp. 12, 418-19, 431-32). It is the scholastic philosophers, however, who, attempting to introduce the categories of a faulty physics into the rules for worshiping God, are responsible for most of the absurd propositions of Catholic doctrine (such as transubstantiation, free will, and "incorporeal substances"). These, in Hobbes' view, were responsible for all the disputes of the Reformation (*De Homine* XIV.13; cf. *Leviathan,* Ch. 12, p. 79):

> School-divines, who striving to make good many points of faith incomprehensible, and calling in the philosophy of Aristotle to their assistance, wrote great books of school-divinity, which no man else, nor they themselves, were able to understand. . . . For none of the ancient philosophers' writings are comparable to those of Aristotle, for their aptness to puzzle and entangle men with words, and to breed disputation, which must at last be ended in the determination of the Church of Rome (*Behemoth,* pp. 185, 215)

But even if incomprehensible even to themselves, the "distinctions which signify nothing" that are the stock-in-trade of the philosophers do serve "to astonish the multitude of ignorant men" (*Behemoth,* p. 213); even absurdities can be effective weapons of political controversy. Hobbes' other example of this is the case of the common lawyers, who, declining to accept the definition of law as command, seek an alternative definition in "natural reason," "universal reason," or especially "legal reason," defined as the "artificial perfection of reason, gotten [by lawyers] by long study, observation, and experience." But Hobbes finds unintelligible the concept "legal reason": "There is no reason in earthly creatures, but human reason" (*Dialogue,* pp. 122, 22, 4); to essentialize an accident which is properly used only of individuals is a logical error culminating in "insignificant speech," or absurdity, which is yet not without its attractions.

Passionate speech, inconstant signification, equivocation, metaphor, eloquence, absurdity: these then are the main types of abuse of language analyzed and illustrated by Hobbes. But why are these abuses so frequently encountered? In the first place Hobbes presents us with a variety of observations about motivation with respect to speech as an autonomous human activity. Habit, for example, can detract from the rational use of speech: a man can speak partly or entirely without thinking—

> as it is with beggars, when they say their *paternoster,* putting together such words, and in such manner, as in their education they have learned from their nurses, from their companies, or from their teachers, having *no images or conceptions* in their mind, answering to the words they speak (*Elements* I.5.14).

Hobbes also mentions such speech-acts as "cursing, swearing, reviling, and the like" which "do not signify as speech; but as the actions of a tongue accustomed" (*Leviathan,* Ch. 6, p. 39). Nor is it only beggars and swearers who speak from mere habit; learned men also "take up maxims from their education, and from the authority of men, or of custom, and take the habitual discourse of the tongue for ratiocination" (*Elements* I.13.4). In none of these examples of habitual speech does Hobbes examine political implications, yet one can without difficulty understand how the mechanism of a "tongue accustomed" may perpetuate the use, in political controversies, of the various insignificant, equivocal, and otherwise passion-laden terms invented by philosophers and converted to partisan use.

It is also easy to grasp, in the light of Hobbes' onslaught on professional philosophers, the significance of his second psychological observation, which concerns the intrinsic appeal of words to certain kinds of people. Knowledge is said to be "good," not only because it is useful, but because it is pleasing to men's natural curiosity (*De Homine* XI.9), and because "the constitution of man's nature, is itself subject to desire novelty" (*Leviathan,* Ch. 29, p. 213). Language, furthermore, can be beautiful: "innocent" ornamentation is even said to be one of its legitimate uses (*Leviathan,* Ch. 4, p. 19); and beautiful language can be seductive: "The reading of some such books is very delightful; for they are most eloquently written, and contain many clear, wholesome, and choice sentences, which are yet not universally true" (*De Corpore* I.7). And the principal reason why ancient philosophy has for so long held its ground against the moderns is "not so much by giving any light to the understanding as entertainment to the affections" (*De Cive,* Ep. Ded.). It is, of course, primarily intellectuals—"such as love knowledge"—who are attracted to the mysteries of words and elaborate doctrines, and who "consequently take delight in finding out the meaning of the most hard texts . . . [and] praetermitting the easy places which teach them their duty, fall to scanning only the mysteries of religion" (*Behemoth,* p. 232).

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[10] On Hobbes' theory of absurdity, see Peters (1967, pp. 127-28).

[11] *Elements* I.11.5; *Leviathan,* Ch. 45, p. 418. Hobbes uses "phantasms" as generally equivalent to "images" in the mind, the source of which is always physical and external, and which usually but not always reflect an external body. Cf. Peters (1967, pp. 116-18) on private phantasms and public meanings.

Seldom has a philosopher had sharper words for fellow philosophers of differing views than Hobbes has for the scholastics, yet in this antagonism he is merely illustrating his final major point on the psychological motivation of disputes with words:

> Furthermore, since the combat of wits is the fiercest, *the greatest discords* which are, must necessarily arise from this contention. . . . For not to approve of what a man saith, is no less than tacitly to accuse him of an error in that thing which he speaketh: as in very many things to dissent, is as much as if you counted him a fool whom you dissent from. Which may appear hence, that there are no wars so sharply waged as between sects of the same religion, and factions of the same commonweal, where the contestation is either concerning doctrines or politic prudence (*De Cive* I. 5, emphasis added).

Man's nature is to seek power; but glory is the sense of power, and fine speech is a source of glory (*De Cive* I.2); while honor is the recognition of one's power by others, and so men quarrel "for trifles, as a word, a smile, a different opinion, or any other sign of undervalue" (*Leviathan,* Ch. 13, pp. 81-82). Words, as slander, can serve as direct weapons of attack on another's honor, provoking "brawling and fighting"; and from the fact (contrary to Hobbes' usual estimation of the fear of violent death) that "most men would rather lose their lives, . . . than suffer slander" (*De Cive* III.12; *Leviathan,* Ch. 15, p. 100) follows a law of nature against contumely. But words are weapons indirectly as well, as men fight with doctrines as a mode of competition for honor. Hobbes says that men do not quarrel over geometry because no interest is involved (*Leviathan,* Ch. 11, p. 68), yet he himself was for years engaged in a bitter dispute over a mathematical demonstration, thus confirming his observation: "There is scarce any principle, . . . from whence there may not spring dissensions, discords, reproaches, and by degrees war itself," a fact attributable to "the disposition of men, who, seeming wise to themselves, will needs appear such to all others."[11] Hobbesian men (like Hobbes himself) are attracted to words and demonstrations; they devise doctrines in which they come to feel a vested interest, and they defend them with all the passion elicited by their instinct for honor and eminence. When Hobbes claims that knowledge is power, his meaning goes beyond a Baconian orientation to utility to a recognition of a separate plane of human intercourse and conflict on which verbal confrontations are primary.

If Hobbes' analysis of language provides much insight into the dynamics of doctrinaire disputes, in which people are divided more over words than over facts and material interests, he is also interested in those cases where words—still employed as political weapons—serve and sometimes mask group interests of various kinds. Hobbes thus on occasion articulates, although not in any systematic fashion, a concept of ideology as a second general way of accounting for language abuses (cf. Wolin, 1961, p. 475, n. 45). One can find a few suggestions that a crude acquisitive instinct normally underlies ideological conflicts: Hobbes concludes generally about the Church-state clashes, for example, that "the Kings had a more considerable part on their sides, than the Pope had on his; and shall always have so, if they have more money" (*Behemoth,* p. 186). Doctrinaire differences are likely to occasion violent conflict only when they coincide with the "ambition, profit, or lust" that Hobbes elsewhere asserts underlie all disputes (*Leviathan,* Ch. 11, p. 68). Likewise, Hobbes alleges that men who invent clever doctrines do so mainly because it is lucrative: "Astrology . . . is the strategem of a man to steal money from stupid people in order to escape poverty. Those who pretend that they are prophets, when they have performed no miracles, have the same purpose'' (*De Homine* XIV.12). And his history of the Civil War is replete with allusions to the financial interests lying behind the proceedings of the Commons, as when he points out that the Parliamentarians, after having willfully misinterpreted Magna Carta to support their disinclination to pay Ship Money, proceeded when in power to tax the country, in their own interests, even more heavily than the king had done (*Behemoth,* pp. 209-12).

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[11] *De Cive* VI.11 (note). Hobbes' dispute with Wallis on squaring the circle is discussed in Peters (1967, pp. 36-38). Cf. Wolin (1970, pp. 15-16) on Hobbes' own "epical impulse" to intellectual combat; and Oakeshott (1975, p. 12).

More often, however, Hobbes identifies a concern for honor or a drive after power itself, rather than material interest, as the motivating force. He doubts, for example, that the aristocratic Scots Presbyterians could have had such tender consciences or fine theological convictions, but rather finds that their religious allegiance was a suitable vehicle for asserting their particular Scottish and aristocratic claims to honor (*Behemoth,* pp. 200-01). Ambition for office and power is imputed to democrats—''seditious teachers, and other prating men''—whose verbal abilities serve them as weapons to the end of ''popular government, where the most ignorant and boldest talkers do commonly obtain the best preferments'' (*Dialogue,* p. 18). It is to the corporate interests of ''unpleasing priests'' of all sects that ''all the changes of religion in the world'' may be attributed (*Leviathan,* Ch.. 12, pp. 79-80)—priests who ''wish to win people over not so much to faith in God (in whom all believe already) as to faith in themselves'' (*De Homine* XIV.4). And to this end the mere appearance of knowledge can be an effective weapon; for while some religious men go so far as to feign inspiration and prophecy, ''the rest pretend only, for their advancement to benefices and charge of souls, a greater skill in the Scriptures than other men have, by reason of their breeding in the universities'' (*Behemoth,* p. 276). Philosophers, finally, do not escape the charge of rationalizing, in their doctrines, a desire for power and influence:

> Aristotle in the first book of his *Politics,* for a foundation of his doctrine, maketh men by nature, some more worthy to command, meaning the wiser sort, such as he thought himself to be for his philosophy; others to serve, meaning those that had strong bodies, but were not philosophers as he (*Leviathan,* Ch. 15, p. 100).

The love of novelty and eloquence, the attraction of mysteries, the inertia of received opinions institutionally entrenched—if all of these are insufficient to account for so startling an array of abuses, Hobbes is never slow to suspect and to unmask the ''restless desire of power after power'' which is so often its most fundamental motivation.

The Right Uses of Language and Hobbes' Objectives

The extent of the possibilities of the misuse of speech, and the universality of the factors that encourage it, would appear not to augur well for Hobbes' objectives of political science and civil peace. Can Hobbes entertain any serious hopes in these respects when he is generally so pessimistic —and so incisive in his documentation of the grounds for pessimism—in others? His case rests on the conviction that language abuses and fallacies are of such a nature that, once they are uncovered, they can be abolished; and philosophy is charged with this task. The feasibility of this project, however, can be challenged on grounds provided by Hobbes himself; and in the final analysis it seems that his case is too strong, that he himself provides, on numerous occasions, insights which implicitly belie his ostensible purposes. First, however, I shall consider two Hobbesian prototypes of the correct use of language, which stand as counterparts to the perversions already discussed: counsel in the realm of speech, and science in that of doctrine. Hobbes discusses these two enterprises as examples of the legitimate and proper use of words, and they provide models of the rationality whose implementation he seeks. Even as we examine them, however, it is difficult to escape the sense of precariousness with which they are presented; and they may thus serve as an introduction to the criticisms of Hobbes' position with which I shall close.

Hobbes points out that confusion arises from the fact that three different modes of speech make use of the ''imperative manner of speaking,'' and that the same words, ''do this,'' depending on the context, the speaker, and his motives, may constitute either command, counsel, or exhortation. The nature of command is clear: it depends on the presence of obligation in the addressee. As between the other two, Hobbes seeks clearly to distinguish (legitimate) counsel from exhortation, or ''counsel vehemently pressed.'' Counsel is dispassionate speech whereby the experience of the counselor is made known to another—in a polity, to the ruler—in ''as significant and proper language, and as briefly, as the evidence will permit'' (*Leviathan,* Ch. 25, pp. 166-69). Since experience is identical with memory of past consequences, which is vital to the formation and confirmation of probable propositions and predictions, a good counselor is said to be analogous to the memory and reason of the ruler he serves; in fact, Hobbes regards counsel as an exemplar of legitimate speech because it is simply an extension of the rational faculties, enlarging the ruler's experience and assisting in his reasoning from cause to effect, and from end to means.[13]

Counsel thus understood presupposes the absence of passion or interest—indeed of will—on the part of the counselor; the end of counsel is the benefit of the ruler of whose reason the counselor is an extension. That this is an unattainable ideal, however, is suggested when Hobbes says that in practice no counsel can ever be entirely free of passion (*Elements* II.5.4). What purports to be counsel readily shades over into its perverted, "vehement" counterpart, exhortation or dehortation. These, while attempting to maintain the appearance and forms of ratiocination, are varieties of illicit "passionate speech":

> And, therefore, they have in their speeches, a regard to the common passions and opinions of men, in deducing their reasons; and make use of similitudes, metaphors, examples, and other tools of oratory, to persuade their hearers of the utility, honor, or justice of their advice (*Leviathan,* Ch.. 25, p. 167).

Besides the "long and vehement urging" that is invariably its mark, Hobbes argues that proffering unwanted advice (such as Parliament directed at an unwilling king) amounts to exhortation, and in this form it is prohibited by a law of nature (*Elements* I.17.8). Giving false "counsel" may of course be motivated by the counselor's own selfish interests; but it may also be pressed on an unwilling ruler, because, as with public speaking in general, it can be a source of honor: "To hearken to a man's counsel, or discourse of what kind soever, is to honour; as a sign we think him wise, or eloquent, or witty. To sleep, or go forth, or talk the while, is to dishonour" (*Leviathan,* Ch. 10, p. 58). The omnipresent urge to eminence which operates when a man is among other men, and the suitability of speech to this end, leads Hobbes to deny the possibility of genuine counsel being given to a multitude and to suggest that a sovereign listen to counselors separately, never in an assembly, where all the dangers of passion and eloquence converge:

> There cannot be an assembly of many, called together for advice, where there be not some, that have the ambition to be thought eloquent, and also learned in the politics; and give not their advice of care of the business propounded, but of the applause of their motley orations, made of the divers colored threads, or shreds of authors. . . . For the passions of men, which asunder are moderate, as the heat of one brand; in an assembly are like many brands, that inflame one another, especially when they blow one another with orations, to the setting of the commonwealth on fire (*Leviathan,* Ch. 25, pp. 170-71; cf. also *Elements* II.5.4).

If Hobbes offers counsel as a possible type of precise, rational speech, he nevertheless impresses upon us in the same breath its tendency to degenerate into its opposite, leaving us with a vivid picture of the dangers of exhortation. And perhaps even Hobbes fails to perceive the full extent of the potentialities for discord inherent in the offering of advice on policy. Hobbes is able to characterize counsel both as dispassionate reasoning and as advice oriented towards the ends of the ruler, since the counselor is conceived as an extension of the ruler's reason. Conversely, Hobbes describes exhortation both as passionately offered advice and as "corrupt" counsel, covertly in the interest of him who exhorts, since he regards the passions as directly reflecting the appetites and aversions of the individual (*Leviathan,* Ch. 25, p. 167). But this scheme overlooks a third case, which in times of political upheaval and disagreement can be the most discordant of all—the case of the intransigent idealist who passionately advances an opinion which he believes to be directed towards the common (or the sovereign's) good, and which he insists on advancing whether or not the sovereign willingly hears him. For Hobbes, such selfless political activism would count as a special case of exhortation—albeit one that presents peculiar difficulties for Hobbesian psychology.

Hobbes presents his political works as an attempt to construct a comprehensive and exact political science, deduced on the one hand from his materialistic psychology and ultimately from his physics, and verified in many respects on the other hand by reference to experience. Such a science has more than a theoretical interest: for anyone who, like Hobbes, desires civil peace it results in a set of recommendations constituting counsel on a large scale for sovereigns and people alike. The thrust of this project is twofold. The first task of Hobbes' science, since it is a novel discovery, is to combat the false philosophy which has in the past been conducive of erroneous political science and hence sedition: "It is to this purpose, that men may no longer suffer themselves to be abused by them, that by this doctrine of *separated essences,* built on the vain philosophy of Aristotle, would fright them from obeying the laws of their country, with empty names" (*Leviathan,* Ch. 46, p. 442). Since all of the causes of the dissolution of commonwealths are false opinions, or structural weaknesses supported by false doctrines (*Leviathan,* Ch. 24, pp. 211 f.), it is clear that the critical role of the new science must be extensive. But beyond this lies the revelation that states themselves are created by men through the use of words: the pacts and covenants in which they originate "resemble that *fiat*, or the *let us make man*, pronounced by God in the creation."[14] If this is true, then clearly the stability and orderliness of any commonwealth depends on the content of those words spoken by its institutors and subsequently acquiesced in by them and by their successors. Any state rests upon a contract, however this may manifest itself in particular instances; and civil peace in the state depends both on common agreement about the meaning of the terms used in that contract and on the validity of the doctrines presupposed by its articles. Hobbesian political science establishes these propositions and thus the importance of its own role as a source of accurate definitions and valid doctrine; and it then proceeds to enunciate those truths of human nature and of politics on the basis of which the desired kind of commonwealth may be founded. Hence, from the potential contributions of accurate science to the establishment of the conditions of civil harmony, there arises Hobbes' sense of purpose and his confident hopes when setting forth the methods of a new political science. Whether such confidence is justifiable, however, is a matter on which some doubts may be cast.

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[13] *Leviathan,* Ch. 25, deals with the nature of counsel; in chapter 30 Hobbes evidently is offering counsel to the sovereign.

If counsel is that legitimate kind of speech which reflects the functions of observation and memory in reasoning, so science is that which reflects the deductive faculty, embracing those forms of reasoning that result in logical truths (cf. *De Cive* XVIII.4). This means that the only kinds of statements proper to science are universal propositions: particular, empirical claims, and the conclusions resulting from syllogisms in which particular propositions serve as the minor premises, are contingent or hypothetical and thus lack the status of universality proper to science. It also means that scientific truth is grounded in the conventional meanings of the words which constitute the subject and predicate of the proposition. Science proceeds by deductive (or "synthetic") "demonstrations," and it thus depends on the clarity and precision of definition of its initial terms from which the body of propositions are derived (*De Corpore* III.2-11). Besides the avoidance of logical fallacies, however, the truth of any science also depends on the truth of its initial definitions, or first principles. While Hobbes believes that certain statements about bodies and motion must constitute the fundamental definitions of any science of the physical world, he recognizes that the first principles of any science cannot be demonstrated, but only claimed as self-evident (*De Corpore* III.9, VI.5, 12); and their truth in the end relies on the agreement of the propositions derived from them with the evidence of experience: "Now when a man reasoneth from principles that are found indubitable by experience, all deceptions of sense and equivocation of words avoided, the conclusion he maketh is said to be according to right reason" (*Elements* I.5.12).

In Hobbes' system political science belongs to the class of sciences of physical bodies, since man, its subject matter, is nothing but a special sort of body; and thus his political propositions are derived from the principles of his psychology and ultimately his physics. Hobbes' hopes for the acceptance of his political science, therefore, depend on agreement about the self-evidence of his first physical principles; different theories of physics or psychology would result in disagreements about politics, and Hobbes cannot escape the difficulties presented by this fact except through his confidence in the eventual victory of the new physics and his conviction that common experience would serve to confirm the validity of his deductions.[15] But Hobbes must also face the criticisms of opponents who deny that political science is or can be a branch of the physical and psychological sciences and who construct arguments that involve deductions from first principles claiming self-evidence of a different sort. Hobbes points out that all agree to the syllogism, "Socrates is a man, and therefore a living creature," because all agree on the respective meanings of "man" and "living creature." But there are those who also argue that, "Sophroniscus is Socrates' father, and therefore his lord," is equally evident. Hobbes denies that it can be considered evident until agreement has been reached on the definitions of "father" and "lord," and in fact he disputes the definitions implied in this deduction (*De Cive* IX.1). Yet Hobbes cannot escape the fact that there were those in his day who argued this way and drew momentous conclusions from such arguments; nor can he deny that their logic is as precise as his. The difficulty lies in the definitions of words and in particular in the first principles or initial definitions from which one constructs one's demonstrations; and Hobbes' own political science seems unlikely to oust its competitors from the field unless its particular definitions should somehow prove more persuasive than all others.

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[14] *Leviathan*, Introduction, p. 5. Wolin (1961, p. 248) interprets Hobbes' covenant as a "highest expression of political creativity." It should be emphasized that covenanting is (formally speaking) simply a matter of pronouncing the right words, as taught by political philosophy. The creativity rests on verbal acuity.

[15] Cf. Watkins (1968, pp. 144-50). Hobbes seems to have believed in the possibility of objective scientific knowledge, so that the meanings of the words used in physics have a different status of conventionality than moral epithets. But disagreements over scientific, no less than over moral, beliefs can lead to political controversy. For a recent critical discussion of Hobbes' science and the place of language in it, see Danford (1980).

Hobbes' project of having his political science accepted as a whole would appear to be a long and arduous one. There are, however, certain subsidiary objectives that he can pursue, which may have the effects of reducing controversy in certain areas and of conducing in a limited fashion to the strengthening of civil authority. He can, for instance, effect a clarification in political argumentation through the example he provides of rigorous logic and consistency in the use of words. Furthermore, even if he fails to demonstrate the logical necessity of his political science through its deduction from physics, he can still have significant sections of it accepted by winning acquiescence in certain of his key definitions—whether such acquiescence comes about through his claims of self-evidence or through authoritative imposition. At the very least he can wage particular battles against logical fallacies and specific misuses of words, ranging over the various disputes that trouble his countrymen.

It is in these lesser campaigns that Hobbes appeals to common usage and to etymology as means of demonstrating the superiority of his definitions. This represents a retreat from his scientific deductions and a gesture towards the claims of opinion, yet it is a tactic which Hobbes pursues with the same concern for unambiguous meanings that characterizes his scientific ideal. On occasion Hobbes, not unlike a modern language philosopher, seeks the true or original definition of a troublesome term by examining the various ways in which it is customarily used, hoping that the revelation of the inadequacies of the received meanings might result in a willingness to accept his version of the true meaning. Hobbes says, for example, that to know whether or not a promise or covenant has been entered into, "is nothing else but to demand whether such words, spoken in such a manner, be by common use and consent of the subjects *a promise* or *covenant*" (*De Cive* XVII.28). Hobbes here assumes that there exists common agreement on the use of certain terms, which may therefore be accepted as satisfactory by the political scientist. When unanimity is not forthcoming, however, as in the case of the terms "word of God" and "church," Hobbes finds it useful to run through a large number of different usages in the process of evolving definitions which he believes to be the most accurate and the most appropriate for political science (*De Cive* XVII.15-21). Likewise, in offering his definition of law as command, Hobbes seeks to refute its erroneous definitions as counsel, or common consent, or right, by a close examination of how these latter words are used in other contexts—an examination which uncovers inconsistencies when they are applied to law (*De Cive* XVI.1-3). Finally, Hobbes enjoys indulging in speculative etymology when it can aid him in buttressing his definitions, presumably hoping that the argument from origins is one that would appeal to his contemporaries: "Etymologies are no definitions, and yet when they are true, they give us much light towards the finding out of a definition."[16] Some of Hobbes' etymologies, such as those of "ratiocination" and "deliberation" (*Leviathan*, Chs. 4, 6, pp. 22, 37), simply enable him to enlarge on his psychology; others, however, are of direct political significance. The original meaning of "tyranny" suggests that its differentiation from "monarchy" is a merely partisan one (*Leviathan*, Ch. 46, p. 447); that of "heresy" serves to diminish the significance of the offense and to unmask the historical interest of the Roman Church in altering the meaning of the word (*Behemoth*, p. 174; *Dialogue*, pp. 97-102). A correct understanding of the original denotation of the term "chancellor," finally, suggests that this office was at one time a merely subordinate, clerical one and serves as ammunition against Sir Edward Coke's claims to the contrary (*Dialogue*, pp. 55-57).

Hobbes' political science, then, appears to hold promise for the advancement of knowledge and for peace. The obvious difficulty is, as I have suggested, that it is merely one system of definitions and deductions—and rather startling ones at that—offered to a public already divided by doctrinal and terminological disputes. Hobbes' definitions have the advantages of precision, consistency, and systematic coherence; yet for all that they cannot be fully demonstrable, nor are they universally self-evident, nor can they avoid head-on clashes with deeply held popular opinions. In these circumstances Hobbes is faced with the task not only of expounding his system but of convincing others to accept it, persuading them of its superiority. Is this an enterprise in which Hobbes can engage without contradiction, without incurring the charge of using language in ways which he himself regards as improper?

Hobbes must *teach* his principles to others, and he recognizes that he must assume the role of educator:

[16] *Dialogue*, p. 80. According to Hobbes' conventionalist view of meaning, origins should not be important; his use of etymologies thus appears to be pursued as a rhetorical or persuasive device.

> Why may not men be taught their duty, that is, the science of just and unjust, as divers other sciences have been taught, from true principles and evident demonstration; and much more easily than any of those preachers and democratical gentlemen could teach rebellion and treason? (*Behemoth,* p. 212).

Teaching is defined as the "begetting in one another the same conceptions that we have in ourselves," through the marshaling of evidence and right reasoning; if evidence is lacking, and one conveys unfounded opinions, one is no longer teaching but persuading (*Elements* I.13.2). Hobbes thus considers teaching to be a third case of the legitimate use of speech, with affinities to both counsel and science; persuasion is a perversion analogous to exhortation. Yet is not such teaching, like counsel, an unrealizable ideal? Hobbes goes on to say that "the infallible sign of teaching exactly and without error, is this, that no man hath ever taught the contrary"—be the contrary view held by ever so few, "for commonly truth is on the side of a few, rather than of the multitude" (*Elements* I.13.3). "Teaching" in the strict sense is thus reserved for the communication of conceptions of absolute clarity and self-evidence, just as counsel and science are held up as pure types of rational discourse. Yet by this criterion Hobbes is certainly not teaching, since his doctrine is new and controversial. Must he then resort to persuasion in order to promulgate his definitions, and will he not be obliged to utilize passionate speech in this effort?

In his major philosophical work Hobbes asserts: "Philosophy professedly rejects not only the paint and false colors of language, but even the very ornaments and graces of the same" (*De Corpore* I.1); it is partly for this reason that true philosophy finds it difficult to dislodge the false opinions of its opponents, especially if they are eloquent. Later, however, Hobbes tempers his confidence in the capacities of pure science to abolish all errors and bring peace; and in the end he acknowledges that reason must reach some compromise with eloquence in the establishment of a commonwealth:

> So also reason, and eloquence, though not perhaps in the natural sciences, yet, in the moral, may stand very well together. For where soever there is place for adorning and preferring of error, there is much more place for adorning and preferring of truth, if they have it to adorn (*Leviathan,* "A Review, and Conclusion," p. 461).

This admission explains the paradox that Hobbes, despite his vociferous polemics against metaphors, eloquence, and other forms of language abuse, is himself one of the most eloquent, and probably the most metaphorical, of political philosophers (Wolin, 1970, p. 38; cf. Oakeshott, 1975, p. 14). "Metaphors, and senseless and ambiguous words, are like *ignes fatui*; and reasoning upon them is wandering amongst innumerable absurdities" (*Leviathan,* Ch. 5, p. 30);

> A man that seeketh precise truth had need to remember what every name he uses stands for, and to place it accordingly, or else he will find himself entangled in words, as a bird in lime twigs, the more he struggles the more belimed (*Leviathan,* Ch. 4, p. 21).

Figures of speech accentuate the dangers of figures of speech; and an extended simile draws us into an awareness of the ensnaring potentiality of beautiful language: "For speech has something in it like to a spider's web, (as it was said of old of *Solon's* laws) for by contexture of words tender and delicate wits are ensnared and stopped; but strong wits break easily through them" (*De Corpore* III.8). As counsel is in practice not always distinguishable from exhortation, so neither is teaching from persuasion; and Hobbes the teacher—the teacher of a science ostensibly characterized above all by its precision of language—finds that he cannot avoid the tactical use of eloquence as a means of access to the mind of his fellow man.

Hobbes' work is plagued throughout by the contradiction between the alleged self-evidence of its principles and the prevalence of the kinds of errors and discords against which those principles militate. If men are capable of governing themselves according to a rational science, then why do Hobbes' prescriptions encounter so many obstacles and antagonists? Hobbes' mood oscillates between confidence in the efficacy of his demonstrations and outraged wonder at the tenaciousness of errors and absurdities; but in the end the astuteness of his observations of irrational features in political behavior overshadows the assumption of rationality required by his system. Science not only acknowledges that it must use eloquence in its battle for recognition; it also discovers, while engaged in the exposure of errors, that there are propensities in human nature which render questionable the entire project of a scientific politics. The attractiveness of myth and custom, the belief in ghosts, and the tenacity of private meanings are three further phenomena that Hobbes acknowledges but cannot finally reconcile with his hopes for a science.

Hobbes believes that ignorance of the correct principles of politics leds to sedition, but he also recognizes that this is a modern phenomenon, true in an age of disputation but not true in all times and places. All that is necessary to be known for civil peace, he says,

> the most ancient sages did judge fittest to be delivered to posterity, either curiously adorned with verse, or clouded with allegories, as a most beautiful and hallowed mystery of royal authority; lest by the disputations of private men it might be defiled (*De Cive,* Author's Preface).

In those pre-Socratic days, when "the science of justice [was] wrapped up in fables," kings did not have to justify the authority which they exercised, nor did the citizens, who "reverenced the supreme power," measure justice by the "judgments of private men," but by the laws (*De Cive,* Author's Preface). Even Hobbes (of all people) can idealize the ancient city and the unreflecting solidarity, grounded in myth, that it enjoyed. He argues, however, that the time of such harmony is irrevocably past. We live in an age of reasoning and disputation in which the state must be founded anew on doctrine so convincing that it can put an end to all erroneous reasonings forever. Science, in other words, purged of every equivocal and emotive element, must accomplish the same end once achieved in opposite fashion, through fable and myth.[17]

Hobbes knows this will be difficult: the requisite knowledge is not easily come by, and in ancient times, he says, it was more feasible to persuade people of absurd beliefs in gods than to know or teach a rational basis of civil obedience (*De Corpore* I.7). Is it likely to be more feasible in modern times—when people in general are so ignorant that "not one perhaps in ten thousand" understands the principles of government? (*Behemoth,* p. 169). Hobbes at any rate understands the appeal of myth and custom to many people, although in the end one wonders whether he underrates such factors. Hobbes even has his favorite ancient myths which he retells in the interests of his political principles—those of Ixion (*De Cive,* Author's Preface), of Prometheus (*De Cive* I.3 n.; *Leviathan,* Ch. 12, p. 70), and of the daughters of Pelias (*Elements* II.8.15; *De Cive* XII.13); these, we may assume, fall into the category of innocent adornment for his arguments.

More noteworthy is Hobbes' awareness of the ways in which religion has been used throughout time to support political institutions. He mentions a variety of appealing arguments for monarchy, for example, which rely on an analogy between a king and God; and he interpolates into *De Cive* an entire chapter of biblical quotations and parables supporting monarchy (*De Cive* X.3, XI). And he ascribes the motive of civil peace to the ancient founders of religions: "They have had a care, to make it believed that the same things were displeasing to the gods, which were forbidden by the laws" (*Leviathan,* Ch. 12, p. 76); in the past, in any case, there is no doubt that such nonrational supports were necessary to the state.

Closely related to myth as a potential source of stability in a commonwealth are the nonrational factors of custom and habit, which, Hobbes says, can render pleasing things that offend when new (*De Homine* XIII.3). Hobbes explains why, after the execution of the king, the abolition of the House of Lords, and the exclusion of those members who opposed the Ordinance of Non-Address, the then-sovereign Rump continued to refer to itself as "Parliament":

> Men may give to their assembly what name they please, what signification soever such name might formerly have had; and the Rump took the name of Parliament, as most suitable to their purpose, and such a name, as being venerable amongst the people for many hundred years, had countenanced and sweetened subsidies and other levies of money, otherwise very unpleasant to the subject (*Behemoth,* p. 360).

Hobbes thus recognizes that customary and emotive attachments, even to mere names, can be a force in politics. In marked contrast to later conservative theorists, however, he invariably sees them as disruptive forces which must be exposed and done away with by science, and never as sources of stability and peace. Hobbes points out that it was necessary to institute a sign (circumcision) so that Abraham and his descendants would remember their covenant with God (*De Cive* XVI.3), but he does not discuss a possible role for signs in perpetuating among citizens a memory of their civil contract. With obvious curiosity he devotes several pages to coats of arms and archaic titles as signs of honor (*Leviathan,* Ch. 10, pp.. 61-63), yet in general he makes little of the functions of such symbols in political life, except to say that they must fall under the authority of the sovereign. A disposition to rely on tradition and custom is said, like sedition, to follow from ignorance of causes (*Leviathan,* Ch. 11, p. 67). And when the common lawyer proposes that there ought to be in a polity a "reverend respect to customs not unreasonable," Hobbes' retort is that the claims of custom must stand or fall according to reason alone (*Dialogue,* pp. 58-63).

Hobbesian science then is directed with full force against all the mythical, customary, and symbolic elements which he realizes were once necessary foundations for states but which he regards in the present as hindrances to peace; but although Hobbes can expose the greater part of such elements as resting on various kinds of language abuse, he cannot offer any reasons to believe that their force among people is subsiding. The same can be said of another characteristic of human nature, the belief in "ghosts" and the inferences that people commonly draw from spiritual experiences for the conduct of their lives. Ghosts for Hobbes, as we have seen, are mere verbal absurdities, and alleged experience of them is ascribed to various freaks of sensation coupled with ignorance. And yet people, as Hobbes knows, seem to have a strong inclination to absurdities of this sort. Their natural curiosity, aided by imagination, leads them to seek causes, but their ability to account for everything materially is limited; meanwhile phantasms occurring in dreams or in "hallowed places" at night (though actually nothing but motions in the brain) are taken for subsisting things—especially by guilty, fearful, or superstitious men—and identified as ghosts (*De Corpore* XXV.9). And from the belief in ghosts there arises to challenge the civil sovereign a ghostly authority, "working on men's minds, with words and distinctions, that of themselves signify nothing, but bewray by their obscurity" (*Leviathan,* Ch. 29, p. 215). Hobbes' system depends on a "disenchantment" of the world, because otherwise the fear of violent death would remain secondary to the fear of ghosts, and the sovereign edifice would crumble; and Hobbes says that fear of other men is commonly greater than that of "spirits invisible" (*Leviathan,* Ch. 14, p. 92). Yet elsewhere Hobbes contradicts himself, conceding that the civil authority will always be troubled by the claims of a spiritual party, "because the fear of darkness and ghosts, is greater than other fears" (*Leviathan,* Ch. 29, p. 215).

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[17] But see Wolin (1970, pp. 4-8) and Oakeshott (1975, pp. 14, 150-54), who argue that Hobbes' political philosophy, despite its scientific pretensions, should be understood as an epical "thought-deed" or as a new myth, compelling in part because of Hobbes' artistry and skill with words.

Hobbes' ambition to ground the principles of civil authority in science is partially undermined, not only by the persistence of superstition and fear, but by his own admission that there are other means than science by which men can apprehend truth. Christians, he grants, must "acknowledge" things (such as angels, and the soul) which they cannot know (*Elements* I.11.5)—although he does not explore the political implications of this faculty of acknowledgement. And he even makes a place for mystery: there are some things in God's word, he says, which, being above reason, we are not meant to understand, but rather "we are bidden to captivate our understanding to the words" (*Leviathan,* Ch. 32, p. 242). Mystery, then, founded in words not expressive of conceptions, cannot be entirely abolished; and if people have and must have this capacity to be captivated by mystery in religion, there seems to be little reason to suppose that captivating and non-scientific language will cease to be efficacious in other areas of life. The belief in ghosts and the sense of mystery, then, are two further phenomena of human nature which diminish our confidence that Hobbes can successfully clear the field of political discourse of such abuses as elaborate and insignificant language.

It is, finally, possible to criticize the assumption, underlying Hobbes' advocacy of a public language whose key words would be authoritatively defined, that public meanings can ever significantly replace private ones. Hobbes insists, as mentioned above, that the sovereign must set down rules for the use of certain words, and in particular moral epithets, over which controversies arise. But even if the sovereign could enforce in public certain kinds of word usage, it seems unlikely that it could command unanimity of private understanding. Individuals' estimation of their own intelligence, and the diversity of their constitutions, are too great: "After the Bible was translated into English, every man, nay, every boy and wench, that could read English, thought they spoke with God almighty, and understood what he said. . ." (*Behemoth,* p. 190). And moreover, the state has no way of entering a person's mind and examining the meanings which he ascribes to words in private; Hobbes recognizes that the attempt to enforce uniformity of thought, apart from expression, would be futile: "A state can constrain obedience, but convince no error, nor alter the mind of them that believe they have the better reason" (*Behemoth,* p. 242). So it would seem that Hobbes must grant that private meanings will continue to exist, even after public meanings have been laid down, and this will remain a potential source of sedition and conflict. More important, however, is the fact that the sovereign cannot simply create a public vocabulary *de novo,* but must rather attempt to redefine words previously in use; and extant words, while they may be ambiguous and productive of quarrels, nevertheless have meanings. Hobbes may have a point in arguing that justice cannot be said to exist before there is a sovereign authority to constitute what is "just" by its actions, but this does not mean that the word "just" was not used prior to the creation of the sovereign. Nor does it seem correct to suppose that every individual used it in a different sense: more likely the private meanings of the word fell (and would continue to fall) into several categories, perhaps corresponding to factional divisions in society.[18] Thus, not only would private meanings persist after the institution of a public language, but so also would a non-official, and fragmentary, social vocabulary, the availability of which would constitute a continuing source of potential criticisms of authority.

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[18] If meanings were entirely private, of course, they would not be a political problem, since sedition requires the collective efforts of groups of people who understand one another's words.

Hobbes unwittingly provides examples of this possibility, again revealing insights into human nature that serve to weaken his case for political science. It may be established, for example, that "tyrant" is a meaningless term, and that any ruler by right is to be called "king"; otherwise, an "enemy" (*De Cive* XII.3). But how does one identify an "enemy"? It seems that only the terminology, and not the problem, has been changed. And will not equally emotive alternatives for "enemy" be available, if the need arise? Similarly, "just" and "unjust" have no private meaning: "Legitimate kings make things just by commanding them. . ." (*De Cive* XII.1). But will private judgment not be invoked to decide on the meaning of "legitimacy," or if not this word, then some term used in its definition? Even more strikingly, Hobbes allows that anyone may rightfully refuse to obey a "shameful" command of his sovereign, such as the command to kill one's parents, because a parricide would be "hated of all the world" (*De Cive* VI.13). Here Hobbes clearly implies that there exist private criteria for such a quality as "shamefulness," and that it is one on which there is common, though not public, agreement. Finally, there is the problem that, while a Hobbesian sovereign can do no injury or injustice, he can sin (by breaking the law of nature);[19] and while a subject cannot rightfully speak evil of a king, even if he sin (*Dialogue,* p. 139), it would seem that he can think evil of him, and that a standard of sin remains to society as a criterion of judgment over and above that of justice, even if it may not be publicly invoked.

Thus, notwithstanding his critical analysis of verbal disputes and their contribution to political controversy, and despite his hopes of replacing language abuses with the precision of the new political science, Hobbes gives us at the same time indications of the areas in which his enterprise is most vulnerable. If human nature were so tractable, or people so (potentially) rational, that Hobbes' project could succeed, it would probably not be necessary. Instead, however, politics and people's use of language in political life—together with the related pheomena that Hobbes deplores—show every sign of continuing to reflect the contentious features of human nature that Hobbes' writings capture so well.

Although he feared and attacked the proliferation of private opinions on political matters, Hobbes nevertheless, though a private man, sought to teach his fellow subjects "the grounds of their duty, and the reasons why calamities ever follow disobedience to their lawful sovereign" (*Behemoth,* p. 343). Hobbes did this because he believed he was living in a period of unusual disorder, when "reverence for antiquity" was newly declining (*Leviathan,* "A Review, and Conclusion," p. 466) and when common meanings had been lost in a storm of doctrinal conflicts. For us, however, there is nothing extraordinary about this state of affairs, which appears rather to be a permanent condition of modern political life. While Hobbes remains an illuminating guide to this aspect of politics, we must live with the fact, contrary to his hopes, that political philosophy's task of clarification in speech and reasoning is an endless one.

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[19] *De Cive* VII.14, XIII.16, XV.18. Less is made of this possibility in *Leviathan,* where the equation of sin and crime is dwelt upon.

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Black Consciousness and Political Participation: The Missing Link

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Recent research has demonstrated that black Americans are far more politically active than whites of similar socioeconomic status. The difference has been related to black consciousness. Yet the reasons for this relationship have not been adequately explained. Starting with the work of Gurin and Gamson, this article theorizes that black consciousness contributes to political mistrust and a sense of internal political efficacy which in turn encourages policy-related participation. The relationship between the two attitudes and policy-related behavior is demonstrated to be conditional. The conditions favor blacks more than whites. What I shall call the Gamson-Gurin thesis is supported by data from Verba's and Nie's 1967 survey of the American public. The thesis, and its derivations, prove useful in clarifying the scope and nature of black participation in the American political process as well as helping us to understand how individuals in general select one mode of participation over another and how the choice varies by race and social class.

Low-income groups are generally politically inactive. This is particularly true of more demanding forms of political participation, such as campaign work and organized community activities which require scarce human resources and high levels of individual initiative (Verba and Nie, 1972, pp. 133-37; Milbrath and Goel, 1977, pp. 43-74). The poor generally lack the ability and the motivation to participate in politics. Yet, despite their over-representation among the poor, black Americans largely overcome these disadvantages. A large number of studies, using a variety of indicators and samples, report black participation at a level significantly above that which would be expected, given blacks' average level of education, income and occupational prestige, and at a higher level that that of whites of similar status.[1] The evidence suggests a level of black political participation since the 1960s which is historically unprecedented in black history and which rivals, if it does not surpass, that of other major groups in American society.

The incidence of greater black political participation is not uniform. It varies by type of activity and region and, in some studies, it is more prominent among women than men. The traditionally low black voter registration and voter turnout in the South, which continues to be lower than the national average, and the steady decline in black voting outside the South since its zenith in 1964 are major exceptions (Cavanagh, 1979, pp. 11-14). So too is the reluctance of blacks to engage in one-on-one personal contacts with public officials (Verba and Nie, 1972, pp. 164-71). Nevertheless, blacks are more politically active than whites of comparable socioeconomic status in a wide range of activities which have a considerably more demanding and more significant impact on the participants' lives and the local political process.

The most visible forms of black activism are the unconventional confrontation tactics, both nonviolent and violent, which jarred the national consciousness in the 1960s. However, the attention such tactics have received has concealed a much more prevalent involvement among blacks in conventional politics at the local level. To a degree disproportionate to their numbers, blacks have been involved in numerous community activities —including Community Action Agencies, Model City and Head Start programs, civil rights organizations, block associations, neighborhood development projects, tenant associations, PTA and related neighborhood school groups—as well as more overtly partisan activities such as belonging to political clubs, attending political meetings and rallies, donating money and time to partisan campaigns, and attempting to influence the opinions

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[1] Much of the research on black Americans is based on case studies covering a wide variety of people, places and times. Sources reporting a higher level of participation among blacks include: Babchuk and Thompson, 1962; Matthews and Prothro, 1966, pp. 49-50; Orum, 1966; Olsen, 1970; Zurcher, 1970, p. 103; Verba and Nie, 1972, pp. 151-60; Aberbach and Walker, 1972, pp. 30-34; Williams et al., 1973, p. 13; Greenberg, 1974, p.. 42 f.; Antunes and Gaitz, 1975; Steggert, 1975: Clemente and Sauer, 1975; London, 1975; Lamb, 1975, p. 109; McPherson, 1977: Cohen and Kapis, 1978; Klobus-Edwards et al., 1978.

of others. Some of this participation involves electoral politics. More of it centers around civic projects at the neighborhood level and administrative politics involving dealings with agencies which serve the poor. Much of it has come about as the result of efforts toward political decentralization and community self-government in the 1960s (Lamb, 1975, pp. 4-5).

Verba and Nie (among others) explain the overrepresentation of black Americans among political activists in terms of "black consciousness," the awareness among blacks of their shared status as an unjustly deprived and oppressed group (Verba and Nie, 1972, pp. 160-61; Verba, Ashmed and Bhatt, 1971, pp. 25-26). They demonstrate that blacks who are aware of their oppression are much more active than blacks who are not, and more active than whites of similar socioeconomic status. Even more impressive is the Verba-Nie finding that blacks with a sense of black consciousness participate in campaign and cooperative activities at a rate higher than that of the whites in their sample, despite the clear socioeconomic advantage of the latter. Verba and Nie also identify an increase in black political participation during the 1960s (p. 252 f.), a period in which blacks become increasingly more aware of the illegality of their oppression.

Overall, the empirical case made by Verba and Nie establishing a relationship between black consciousness and political participation is impressive. Yet their theoretical explanation of the relationship, which relies primarily on Olsen (1970), is deficient. It leaves three fundamental questions unanswered.

First, Verba and Nie do not adequately explain why blacks specialize in certain types of activities and not others. Their argument that black consciousness contributes only to collective political action is not supported by their data (Verba and Nie, 1972, p. 164; Olsen, 1970, pp. 692-94). The fact that blacks continue to experience intimidation and a sense of futility when voting and when trying to contact public officials (Verba and Nie, 1972, pp. 164-71) also does not provide a complete answer because it only accounts for why blacks participate in some political activities at a level below that of whites. It does not tell us why they are considerably more active than comparable whites in other areas such as campaign work and community projects. Campaign work and community activities are not necessarily segregated, so neither can the answer lie in the explanation that blacks are compensating for exclusion from white-dominated activities by overparticipating in all-black activities. Second, why do blacks emphasize *political* solutions to their problems? Olsen's explanation, that the black community stresses "participatory norms," group pressure to participate in politics to achieve collective goals, simply puts the question back one step. Why do blacks, but not poor whites, stress participatory norms? What has politicized them? Third, what about poor whites who are politically active? Do they require a different set of motivations to participate in politics? Or does black consciousness give blacks a greater impetus to participate because it contributes to a common set of attitudes which motivate all poor people, regardless of race?

Answers to these questions require that we specify the psychological state linking black consciousness with political involvement. It is the purpose of this article, in drawing upon the work of Patricia and Gerald Gurin and William Gamson, and in extending the analysis of Verba's and Nie's data, to develop such a link.

The Missing Link: Political Mistrust and a Sense of Political Efficacy

The thesis of this article is that the primary reason black consciousness has such a dramatic effect on political participation is that it contributes to the combination of a sense of political efficacy and political mistrust which in turn induces political involvement. The development of this line of reasoning requires that a relationship be established between black consciousness and the combination mistrust and sense of efficacy, on the one hand, and between the combination mistrust and sense of efficacy and political involvement on the other.

It has been established that various mixes of the sense of political efficacy and political trust contribute to different types of political activity (Gamson, 1968, pp. 46-48; Muller, 1977; Milbrath and Goel, 1977, pp. 57-74). The consensus is that a high sense of political efficacy encourages active political involvement: *allegiant activity* through conventional modes of access to government when political trust is also high; critical *protest activity* of an unconventional, nonviolent or violent nature when trust is low. Low levels of the sense of political efficacy discourage political participation, leading to *complacent inactivity* when trust is high and *estranged inactivity* when trust is low.

Gamson (1968, p. 48) contends that the optimal combination for political mobilization is low political trust and a high sense of political efficacy. What underlies his argument is the model of rational actors who decide to act, in part, on the basis of whether they perceive a possible benefit for their effort and what they believe to be the likelihood of success (Riker and Ordeshook, 1973, pp. 45-52). Gamson describes the absence of political trust as providing the "need to act" and the sense of political efficacy as the perceived "ability to act." He states that mistrusting or cynical individuals who nevertheless have a strong sense of political efficacy have both the motivation (the belief that public officials will not be responsive if left to their own devices) and the confidence (the belief that action taken to make public officials responsive will be effective) which are necessary for active political participation. As we shall see, the logic is oversimplified; but, with several refinements, it can be extended to cover a wide range of political activities. Henceforth I shall refer to the principal assertion of Gamson's thesis as the "mistrust–sense of efficacy" hypothesis.

The link between black consciousness and the combination of mistrust and the sense of efficacy is addressed by Patricia Gurin and associates (1969, 1975). We live in a society in which most people accept the Protestant work ethic, the idea that each individual is responsible for his or her own successes and failures in life. This ethic weighs particularly heavily on the poor and racial minorities at the bottom of the socioeconomic hierarchy who traditionally have been shackled with guilt for their poverty. The essence of Gurin's work is the generalization that, if deprived individuals can successfully externalize this guilt, transferring responsibility for their lot from the self to "the system," they will acquire a healthier, improved self-image along with a more critical, cynical view of the system. By drawing attention to their status as a deprived and oppressed group, black consciousness helps blacks to replace the more negative overtones of the Protestant work ethic with a "system-blame" ethic and complete such a transfer. The result is a heightened sense of personal efficacy and an increased mistrust of the political system. This logic is empirically supported by concomitant increases during the 1960s in black self-esteem, self-confidence and political cynicism (Hulbury, 1975: Yancey et al., 1972; ISR, 1973).

The Conditional Relationship between Mistrust–Sense of Efficacy and Participation

Though the work of Gamson and Gurin provides a sound basis for establishing a theoretical link between black consciousness and black political participation, a problem remains. An impressive accumulation of research supports the original mistrust–sense of efficacy hypothesis for acts of political protest; but, so far as I am aware, only for unconventional acts of protest of a violent or nonviolent nature.[3] Rarely do these studies test the hypothesis for purely conventional forms of political participation of the type examined by Verba and Nie, activities such as voting in elections, working in political campaigns or more routine involvement in community politics. More perplexing, those that do, have not substantiated the hypothesized relationship (Muller, 1977, p. 460; Hawkins et al., 1971; Frazer, 1970).

The hypothesis is supported by limited evidence because the three variables—sense of efficacy, trust and participation—have not been recognized as multifaceted, global concepts having different relationships with one another depending upon which facets or components are involved. We should thus identify key components and clarify these relationships before specifying the conditions under which the sense of political efficacy and political mistrust jointly effect conventional participation. Several key assumptions require close attention. The assumptions will be discussed under five topic headings, each heading specifying the points in contention.

1. *Allegiant Behavior Versus Policy Behavior.* There is no reason to believe that political mistrust and the sense of political efficacy have the same relationship with all modes of political activity. A classification of activities is needed. Two criteria are important. The first is the amount of *individual initiative* required for the activity. It is generally agreed that actions like flying the flag, voting and discussing politics with friends require

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[3] Some typical works are: Paige, 1971; Millbrath and Goel, 1977, pp. 68-74; Caplan, 1970; Crawford and Naditch, 1970; Forward and Williams, 1970; and Sanger and Alker, 1972. There are, however, what I interpret to be confirmations of the hypothesis for conventional participation from a related body of literature dealing specifically with system blame and the sense of personal efficacy. In his analysis of the Detroit area study, Aberbach (1977, p. 558) reports that individuals who blame their lack of political efficacy on the political system are more active in *both* conventional and unconventional political activities than those who blame it on their own personal inadequacies. Miller et al. (1978, pp. 13-14) find an interactive effect on political participation between "group identification" (related to the group consciousness predicted here to contribute to internal political efficacy) and blaming the system. Klobus-Edwards et al. (1978) find blacks who blame the system are more active in four out of five modes of activity, one of which is an index of "organized involvement" which includes political and civil rights organizations.

Though Klobus-Edwards et al. find dissimilar results for blacks on an index of "political involvement" (which includes voter registration and turnout) and for higher-status whites on all indices of participation, their results are consistent with the thesis to be developed herein.

considerably less initiative and effort than most forms of campaign work, community involvement and violent protest.

The second criterion is the *purpose* of the activity. The distinction made by Almond and Verba between participant and subject orientations is pertinent here (1963, pp. 13-19). People with only subject orientations make no effort to influence policy other than to support it by their compliance. For them, "good citizenship" typically means obeying the law and showing other displays of loyalty to their regime or country. Activities which fit this mode will be called *allegiant behavior.* The participant orientation entails an active and vigilant role in the policy-making and policy-implementation process. "Good citizenship" is interpreted in terms of popular sovereignty and entails the right and the obligation to exercise influence in public affairs. Any attempt to affect the election or appointment of public officials or the actions of public officials for the purpose of influencing the policy-making or policy-implementation process will be labeled *policy behavior.*

Table 1 presents a partial inventory of political activities cross classified by purpose and level of initiative. The proper classification of activities is sometimes ambiguous. This is owing in part to the occasionally thin line between respondents' loyalty to their regime or country and disagreements over government policy (for example, during the Vietnam War people tended to confuse disputes with a government policy with disloyalty to country). It is also owing to the multiple purposes served by some activities such as voting. Although Verba and Nie (1972, p. 52) and Milbrath and Goel (1977, p. 12) consider voting to be largely a low-initiative allegiant activity, it must be recognized that the meaning and difficulty of the act vary, depending upon the individual.[3] Because

Table 1. Classifying Political Activities by Their Purpose and by the Level of Initiative Required to Implement Them

	PURPOSE	
INITIATIVE	Allegiance to Regime and Country	Influence on Government Policy
HIGH	*a* 1. Active participation in patriotic rallies 2. Volunteering for combat duty in the armed forces 3. Individual acts of heroism by citizens, policemen, firemen, or soldiers in support of community or country 4. Nonviolent mass demonstrations in support of country or regime 5. Violent actions (lynching, riots, etc.) intended to display support for the nation and opposition to perceived enemies	*b* 1. Active efforts to educate oneself on the issues for the purpose of casting an informed vote or swaying the opinions of others 2. Involvement in political clubs 3. Active participation in political campaigns 4. Active lobbying of public officials 5. Running for party or public office 6. Active involvement in community organizations concerned with public issues 7. One-on-one contacts with public officials 8. Nonviolent direct action protest against specific government policies 9. Violent protest against specific government policies
LOW	*c* 1. Flying the flag or displaying other symbols of patriotism 2. Passive compliance with the law 3. Deference to government authorities responsible for implementing the law 4. Passive attendance at patriotic rallies, parades 5. Voluntary enrollment in the nation's armed forces 6. Verbal support of country or regime 7. Voting as a symbolic gesture of regime loyalty	*d* 1. Wearing a campaign button 2. Discussing political issues with friends 3. Voting as a choice among competing alternatives

Source: Compiled by the author.

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[3] Voting can be credibly assigned to any one of three cells in Table 2. For many Americans it is a low-initia-

voting cannot be decisively classified, it is impossible to predict which combination of trust and efficacy will optimize its occurrence. Studies which have failed to substantiate the mistrust-sense of efficacy hypothesis for conventional participation are largely based upon voting data and cannot be interpreted as a general refutation of the hypothesis.

Applying this classification to modify Gamson's thesis, we can expect that, first, political efficacy is conducive to high-initiative activities and, second, trust contributes to allegiant behavior, whereas mistrust encourages solely policy behavior. Therefore the relationship between mistrust-sense of efficacy and participation is expected to be limited to *high-initiative policy behavior.* This is exactly the kind of political behavior in which black Americans have been prominently involved.

2. *Internal Political Efficacy Versus External Political Efficacy.* The distinction between internal and external efficacy (Balch, 1974) has been ignored in past tests of the mistrust-sense of efficacy hypothesis. A sense of political efficacy is the expectation that one's political activity will be successful. A *sense of internal political efficacy* is that portion of a person's sense of political efficacy which is perceived to stem from one's own internal strengths or weaknesses. A *sense of external political efficacy* is the portion of the sense of political efficacy which is believed to stem from environmental forces external to the individual that influence the ease or difficulty of successful political action. *Political trust,* which is faith in the ability and will of government to respond to peoples' needs, is one component of the sense of external political efficacy, referring to the regime, institutions and public officials—in other words, to what is collectively referred to as "government." Empirical tests of the mistrust-sense of efficacy hypothesis which ignore the distinction between internal and external efficacy are confounded by the fact that political trust and external political efficacy are related by definition. Gamson's proposition will accordingly be renamed the *mistrust-sense of internal efficacy hypothesis.*

3. *Political Trust: Perceived Need to Act versus the Perceived Ability to Act.* Political trust influences not only the perceived "need" to act, pointed to by Gamson, but also the "perceived ability" to act. Persons who have a high sense of internal political efficacy will be even more confident if they also have a high sense of external political efficacy; that is, if they believe that the political environment is conducive to success (in the case of political trust), that those to whom they seek access are competent and responsive. Most forms of allegiant behavior comprise low-initiative, largely symbolic activity which can be sustained by political trust alone.

4. *The Role of Trust in Conventional Participation versus Its Role in Unconventional Participation.* Individuals may trust one segment of the political system (e.g., incumbents, institutions, regime principles, and the political community) while withholding support from others (Gamson, 1968, pp. 49-50; Muller and Jukam, 1977). As a rule, tests of the mistrust-sense of efficacy hypothesis have ignored this distinction. A global, undifferentiated mistrust is typically said to result in unconventional protest. This assumption does not square with the fact that as long as one has even minimal faith in existing political institutions, it is much less costly to protest against government policy through traditional, conventional modes of participation than it is to engage in unconventional demonstrations of a violent or nonviolent nature. Muller (1972) and Muller and Jukam (1977) have demonstrated that individuals are only likely to resort to violence when all conventional modes of access to the policy-making process are blocked or futile, when the entire system (as opposed to an incumbent administration) is perceived to be unresponsive and incapable of reform and when violence is perceived to be an effective alternative political tactic. Therefore we may expect political mistrust-sense of internal political efficacy to contribute to both conventional and unconventional policy behavior, but more typically to the former.

5. *Ceteris Paribus versus the Presence of Other Motivations.* Much of the relevant literature implicitly assumes that mistrust provides the only necessity to act, that "everything else is equal." At least for most conventional forms of policy behavior, this is clearly incorrect. Various motivations encourage political involvement—civic duty, partisanship, interest in politics, material self-interest, and ideological principles (Verba and Nie, 1972, pp. 82-88; Milbrath and Goel, 1977, pp. 43-85). Mistrust is neither necessary nor sufficient. Consequently, for citizens who are *already politically motivated,* the combination which maximizes an overall sense of political efficacy (high political trust and a strong sense of internal political efficacy) will also maximize high-initia-

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tive, symbolic act intended to display patriotism and good citizenship (cell *c*). For many others it is an attempt to influence policy by choosing among candidates who are perceived to offer viable alternatives. Voting is also one of the easiest ways to attempt to influence the policy process (cell *d*); but, for those who take it seriously, responsible voting takes a considerable amount of time and effort informing oneself about issues, party platforms and the candidates' relative merits (cell *b*).

tive political involvement of both an allegiant and a policy nature. It is only among those groups, such as the poor, who are normally *not otherwise politically motivated* that the combination of political mistrust and a high sense of internal political efficacy optimizes political behavior, and then only policy behavior. Among these people mistrust is a necessary condition for participation in the policy-making and policy-implementation process.

The following six generalizations summarize the discussion refining Gamson's original hypothesis by specifying the conditions under which political trust and the sense of internal political efficacy contribute to political participation. The relationships specified by the six generalizations are displayed in Table 2. The table presents two models: the first assumes no other motivation to participate; the second assumes the existence of other motivations. The symbols in parentheses accompanying each generalization refer to the corresponding model and cell in Table 2.

For our purposes, the key generalization is the last: *The combination of political mistrust and a sense of internal political efficacy optimizes high-initiative, conventional policy behavior for otherwise unmotivated citizens.* Low-income groups are poorly motivated. However, because of the prevalence of black consciousness, black Americans are more likely than other low-income groups to feel personally efficacious, but cynical about politics; therefore they are more likely to participate actively in conventional policy-making and policy-implementation processes.

Summary of Generalizations:

1. Ceteris paribus, a high sense of internal political efficacy encourages *high-initiative* political participation (I a, b; II a, b); the absence of a sense of internal political efficacy discourages such activity (I c, d; II c, d). One's sense of internal political efficacy is of less consequence for *low-initiative* political participation.
2. Ceteris paribus, high levels of political trust contribute to *allegiant behavior* (I a, c; II a, c); mistrust contributes to *policy behavior* (I b, d; II b, d).
3. When citizens limit their cynicism to a low opinion of public officials, they are likely to rely solely on *conventional* modes of political par-

Table 2. The Joint Effects of the Sense of Internal Political Efficacy and Political Trust on Different Modes of Political Participation, Controlling for Prior Motivations

Model I: Given no other motivation to participate

		Trust in Government	
		High	Low
Internal Political Efficacy	High	*a* Optimal High-initiative allegiant behavior	*b* Optimal high-initiative allegiant behavior
	Low	*c* Low-initiative allegiant behavior	*d* Total withdrawal from conventional politics

Model II: Given prior motivation to participate

		Trust in Government	
		High	Low
Internal Political Efficacy	High	*a* Optimal high-initiative allegiant and policy behavior	*b* High-initiative allegiant and policy behavior
	Low	*c* Low-initiative allegiant and policy behavior	*d* Total withdrawal from conventional politics

Source: Compiled by the author.

ticipation. When they extend their cynicism to the entire political regime, they are more likely to use *unconventional* tactics. Whether the unconventional tactics are violent or nonviolent depends upon the citizens' assessment of the relative efficacy of violence.

4. Regardless of motivation, individuals who have absolutely no sense of political efficacy, who have neither a sense of internal political efficacy nor political trust, withdraw from conventional politics altogether (I d, II d).
5. For otherwise *motivated individuals,* conventional participation of either a policy or allegiant nature occurs when either a sense of internal political efficacy or political trust is present (II, a, b, c). The optimal combination for high-initiative behavior of either type is a strong sense of internal political efficacy and high political trust (II a).
6. For otherwise *unmotivated individuals,* the optimal combination for high-initiative conventional policy behavior is a high sense of internal political efficacy and low political trust (I b). The optimal combination for high-initiative allegiant behavior is a high sense of internal political efficacy and high political trust (I a). Trusting citizens who have little sense of internal political efficacy have no basis for participation in other than low-initiative allegiant activities (I c).

An Empirical Test

If the foregoing discussion, merging the work of Gurin and Gamson, provides a successful explanation of the link between black consciousness and conventional policy behavior, then the following *working hypotheses* should be supported by Verba's and Nie's data: (1) poor blacks willl be more likely than poor whites to possess the combination of mistrust and sense of internal efficacy; (2) the advantage will be limited to blacks who display a sense of black consciousness; (3) among the poor (whether black or white), those who display the combination will participate at a higher rate in the high-initiative conventional policy activities studied by Verba and Nie—campaign and communal participation—than those who do not.

There is no reason to expect the Gurin-Gamson thesis to pertain to individuals of higher socioeconomic status. Generalization 5 indicates that Gamson's hypothesis has no relevance for higher-status, higher-motivated individuals. Gurin's hypothesis does not apply because the transition from blaming oneself to blaming the system for one's plight does not pertain to economically successful blacks. Those who have not failed have no need for blame and therefore nothing to transfer. Nevertheless, in the following analysis I will display the data for both poor and non-poor. Data on the non-poor will provide a frame of reference for understanding the poor and serve as a resource for those interested in a more general theory of participation.

Measurement. The operational definitions for black consciousness and political participation have been replicated from Verba's and Nie's *Participation in America.*[4] *Black Consciousness* is indicated by whether blacks voluntarily raise the issue of race in response to a series of open-ended questions inquiring as to the presence of any conflict within their communities or any problems they perceive in their personal lives, the community or the nation. The spontaneous mention of racial problems and racial conflict among black respondents is treated as an indicator of the potential saliency and importance of racial conflict as an explanation of personal failure. Activities included in the *campaign index* entail persuading others how to vote, working in a campaign, attending political meetings or rallies, contributing money to campaigns, and membership in political clubs. The *communal index* includes efforts to solve community problems by working with others, helping to form groups or organizations and the initiation of personal political contacts within the community. The two indices of participation have been combined into a single *high-initiative conventional policy behavior index* which has been standardized to have a mean equal to zero and standard deviation equal to one.[5]

Composite measures of the *sense of internal political efficacy* and *political trust* have been created by selecting relevant variables in the study and submitting them to several stages of factor analysis, using Thurston's most simple structure criterion. Several weakly correlated or ambiguous items have been discarded; other nearly identical items are combined. The final two-factor solution is presented in Table 3.[6] Items 1 and 5 are composite indices in which the responses to identically worded questions repeated for local, state and national politics are standardized and added together. The first factor is comprised of those questions which best measure a sense of internal political efficacy, each posing the question of political efficacy in terms of a personal pronoun, either referring to the individual or to people like the individual. The second factor contains questions which most distinctly measure political trust, addressing the ease of access to, and the responsibility of, government officials. Weighted composite indices have been reconstructed for each factor and then standardized. The factors are orthogonal, having a Pearson correlation of −.03.

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[4] The full weighted sample of 3095 and the scale construction for the political participation factors and race consciousness are described in the appendices of *Participation in America* (Verba and Nie, 1972). The analysis that follows was repeated for the unweighted sample and no important differences were identified.

[5] A previous report on this project (Shingles, 1978) indicates identical results when the campaign and communal indices are tested separately. For this analysis the 10 individual items comprising the two scales are weighted by their factor scores on the first unrotated factor, added together and then restandardized. Unlike Verba and Nie, I used all the items from the "communal" factor. In their analysis of black consciousness, they exclude the personal contact items because they wish to examine only "cooperative" or group-related activities. The emphasis in this analysis on high-initiative policy behavior requires their inclusion. Their inclusion does very little to alter the relationship with black consciousness reported in *Participation in America.*

Findings for High-Initiative Conventional Policy Behavior.

WORKING HYPOTHESIS 1: *Among the poor, blacks are more likely than whites to possess the combination of political mistrust and a sense of internal political efficacy.* Table 4 presents average standardized scores measuring the sense of internal political efficacy and political trust for each race, controlling for income.[7] As hypothesized, poor blacks are more likely than poor whites to possess the combination of mistrust and a sense of internal efficacy; in fact, they are much more likely to.

Average scores for whites do not deviate very

Table 3. Rotated Factor Loadings* for the Questions Measuring Sense of Internal Political Efficacy and Political Trust

Item	Internal Political Efficacy	Political Trust
1. Two individual efficacy questions combined: Suppose the most powerful people in all of the United States (this community) were at the top of this ladder and the least powerful were at the bottom. . . . Where would you place yourself in terms of power?	.78	−.18
2. Thinking about the important local issues in this community, how well do you feel you understand them?	.63	.18
3. How much influence do you think people like you can have over local government decisions?	.61	.38
4. If you had some complaint about a local government activity and took that complaint to a member of the local government council, how much attention would you expect him to pay to what you say?	.34	.60
5. Three government comprehension questions combined: In general, how well do you think the local (state, national) government understands the needs of the people in this community (state, country)?	.08	.49
6. Suppose you had some problem to take up with an important local official in local government . . . but you did not personally know this official—would it be necessary to go through some connection to approach the official or could you approach him directly? (If a connection is needed) could you easily find some such person who could help you make such a contact?	−.09	.73

Source: Compiled by the author from a 1967 survey of a representative sample of Americans 21 years or older conducted by the National Opinion Research Center.

*Principal component analysis using a varimax orthogonal rotation.

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[6] Exact wording of the questions and their frequency distributions may be found in the ICPSR codebook, *Political Participation in America* (ICPSR 7015). Variable numbers of the items included in the final factor analysis in order of their appearance are v102, v106, v155, v156, v160, v307, v308, v309, v303, v304.

[7] Writing in 1965, Gunnar Myrdal estimated that approximately two-fifths of the American population lived in poverty. The closest coded value to Myrdal's estimate in the 1967 Verba and Nie index of annual family income before taxes is $5,000.. Thirty-seven percent of the sample report incomes of $5,000 or below. This closely approximates the 1968 median family income of $5,200 for families of Target Neighborhoods reported in the OEO funded Local Change Study (Lamb, 1975, p. 59). Therefore "non-poor" are defined as those earning annual family incomes before taxes of over $5,000, "poor" as those earning incomes of $5,000 or less.

much from the population mean, since whites comprise 86 percent of the sample and largely define the mean. Nevertheless, whereas economically better-off whites as a group are slightly above average in their sense of internal efficacy (.09) and trust (.14), the combination which provides them with the greatest incentive to participate, poor whites fall below average on both (-.18, -.19), feeling mildly incompetent and mildly cynical. They consequently have little reason to engage in high-initiative policy behavior. In contrast, the differences for blacks are startling. Regardless of class, blacks have a much higher sense of internal political efficacy than whites, so much so that poor blacks score higher (.36) than moderate- to high-income whites (.09). Better-off blacks score even higher (.53), a half a standard deviation above the population mean. Equally evident is the fact that blacks, regardless of income, harbor extremely cynical views of government, scoring approximately two-thirds of a standard deviation below the national average (-.67, -.64).

The uniform distribution of black political cynicism across classes is not predicted by the Gurin-Gamson thesis, yet it requires little explanation. Black Americans are the victims of a long history of exploitation, discrimination and exclusion from power. In publicizing the black plight, political leaders and a free press have legitimized and intensified black discontent (Hraba and Siegman, 1974). No matter how the black sample is subdivided, mistrust appears to permeate black society (Abramson, 1972; Laurence, 1970).

Even more impressive is the magnitude of black internal political efficacy. How can blacks be so cynical about the responsiveness of government to black people and yet retain so much confidence about their own ability to exercise political influence? The answer lies in black consciousness.

WORKING HYPOTHESIS 2: *Black consciousness contributes to the combination of political mistrust and a sense of internal political efficacy.* Table 5 presents average standardized scores measuring sense of internal political efficacy and political trust for blacks who indicate a sense of black consciousness and those who do not, controlling for income. The hypothesis is confirmed. Among the poor, the combination mistrust–sense of internal efficacy is limited solely to blacks with a sense of their common plight and oppression as a race. Once again, cynicism is uniform across all black groups. However, poor blacks with black consciousness have a remarkably strong sense of internal political efficacy (two-thirds of a standard deviation above the population mean), higher than any other group in the analysis. In stark contrast, poor blacks without black consciousness average no higher in their sense of internal efficacy than the poor whites in Table 4 (-.13 versus -.18). In short, black consciousness accounts for *all* of the differences in the sense of internal political efficacy between poor blacks and poor whites.

WORKING HYPOTHESIS 3: *Political mistrust–sense of internal political efficacy contributes to high-initiative conventional policy behavior.* Part I of Table 6 presents average rates of high-initiative conventional policy behavior (HICPB) for low and moderate-to-high income respondents. Part II breaks the averages down further for different combinations of political trust and sense of internal political efficacy. A two-way analysis of variance (ANOVA) is presented in Part III of the table to provide a test of statistical significance. The multiple correlations in Part IV provide a convenient summary of the strength of the relationship between attitudes and participation.[8]

Table 4. A Comparison of Whites' and Blacks' Average Standardized Scores on the Indices of Sense of Internal Political Efficacy and Political Trust, Controlling for Annual Family Income

	Low Income		Moderate-to-High Income			
	Whites	Blacks	Whites	Blacks	*F*	Sig.
Internal Political Efficacy	–.18	.36	.09	.53	27.033	***
Political Trust	–.19	–.67	.14	–.64	60.290	***
(N)	631	183	1447	117		

Source: Compiled by the author from a 1967 survey of a representative sample of Americans 21 years or older conducted by the National Opinion Research Center.

$^{***}p < .001$

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[8] There are any number of polynomial equations, representing different degrees of interaction which conform to the hypothesis, making the choice of any one model using multiple regression difficult and arbitrary.

Table 5. A Comparison of Average Standardized Scores on the Indices of the Sense of Internal Political Efficacy and Political Trust for Respondents Who Manifest Black Consciousness and Those Who Do Not, Controlling for Annual Family Income—Black Respondents Only

	Black Consciousness					
	Low Income		Moderate-to-High Income			
	Indicated	Not Indicated	Indicated	Not Indicated	*F*	Sig.
Internal Political Efficacy	.66	−.13	.59	.43	6.904	***
Political Trust	−.59	−.80	−.64	−.60	.485	NS
(N)	112	71	78	39		

Source: Compiled by the author from a 1967 survey of a representative sample of Americans 21 years or older conducted by the National Opinion Research Center.

***$p < .001$
NS = not significant

Table 6. A Comparison of Average Standardized Scores on the Index of High-Initiative Conventional Policy Behavior for Respondents of Varying Combinations of Sense of Internal Political Efficacy and Political Trust, Controlling for Annual Family Income

	Low Income[a]		Moderate-to-High Income[b]	
I. Average Standardized HICPB Scores:	−.22		.08	
	Political Trust		Political Trust	
	High	Low	High	Low
II. Average Standardized HICPB Scores by Trust and Internal Efficacy:				
Internal Political Efficacy				
High	−.19	.06	.29	.09
Low	−.26	−.45	−.07	−.22
	F Value	Sig.	*F* Value	Sig.
III. Test of Statistical Significance (ANOVA):				
Main Effects	9.500	***	36.760	***
Internal Political Efficacy	18.897	***	54.754	***
Political Trust	0.320	NS	13.255	***
Interactive Effects	14.326	***	0.217	NS
Total Effects	11.109	***	24.579	***
IV. Multiple Correlation, $R_{y.x_1x_2(x_1x_2)}$:[c]	.33		.39	

Source: Compiled by the author from a 1967 survey of a representative sample of Americans 21 years or older conducted by the National Opinion Research Center.

[a] Total number responding = 1097. Broken down by cell: $a = 246; b = 183; c = 232; d = 398$.
[b] Total number responding = 2016. Broken down by cell: $a = 320; b = 348; c = 497; d = 796$.
[c] y = HICPB; x_1 = sense of internal political efficacy; x_2 = political trust; and (x_1x_2) = their joint effect.
***$p < .001$
NS = not significant

Hypothesis 3 is confirmed. Though in both income groups the magnitude of the multiple correlation between respondents' attitudes and their reported activity are similar, the nature of the associations differ. Among the poor, it is those who mistrust government but feel they can do something about it who are the most involved in high-initiative, conventional policy activities (.06). They participate at a rate equal to that of the average citizen and significantly above that of the poor as a whole (–.22). In short, the combination of mistrust and a sense of efficacy completely overcomes the disadvantages of class. In contrast, among better-off, more motivated respondents, the optimal combination which produces the same behavior is high trust and a strong sense of efficacy (.29). Another way to look at the same relationship is to observe the results of ANOVA. The *F* value for interactive effects (low trust–high sense of internal efficacy) among the poor (F = 14.326) is statistically significant; the *F* value for the non-poor (F = 0.217) is not.

An Aside on the Black Middle Class. Although the Gurin-Gamson thesis does not pertain to moderate-to-high income groups, the small black middle class deserves attention. Their political orientations—in terms of the distribution of political trust and the sense of internal political efficacy (Table 4) and the relation of both to political participation (Table 7)—are very similar to those of

Table 7. A Comparison of Average Standardized Scores on the Index of High-Initiative Conventional Policy Behavior for Respondents of Varying Combinations of the Sense of Internal Political Efficacy and Political Trust, Controlling for Race–Moderate to High Income Respondents Only

	Moderate-to-High Income Blacks[a]		Moderate-to-High Income Whites[b]	
I. Average Standardized HICPB Scores:	.06		.09	
	Political Trust		Political Trust	
	High	Low	High	Low
II. Average Standardized HICPB Scores by Trust and Internal Efficacy:				
Internal Political Efficacy				
High	–.03	.46	.32	.03
Low	–.19	–.31	–.07	–.21
	F Value	Sig.	*F* Value	Sig.
III. Test of Statistical Significance (ANOVA):				
Main Effects	4.988	**	36.702	***
Internal Political Efficacy	7.480	**	48.011	***
Political Trust	2.644	NS	18.434	***
Interactive Effects	2.875	NS	1.694	NS
Total Effects	4.284	**	25.032	***
IV. Multiple Correlation, $R_{y.x_1x_2(x_1x_2)}$:[c]	.36		.40	

Source: Compiled by the author from a 1967 survey of a representative sample of Americans 21 years or older conducted by the National Opinion Research Center.

[a] Total number of respondents = 157. Broken down by cell: a = 48; b = 54; c = 24; d = 29.

[b] Total number of respondents = 1839. Broken down by cell: a = 740; b = 291; c = 469; d = 288.

[c] y = HICPB; x_1 = sense of internal political efficacy; x_2 = political trust; and (x_1x_2) = their joint effect.

**$p < .01$

***$p < .001$

NS = not significant

The results of the most parsimonious equation, ($y = B_0 + B_1X_1 + B_2X_2 + B_3(X_1X_2)$), are presented in Part IV of Tables 6, 7 and 8.

Whereas trust and efficacy are dichotomized for[1] ANOVA, they are left uncollapsed in the calculation of the multiple correlation.

the black lower class. Like the black poor, but even more so, they have an impressively strong sense of internal efficacy, significantly stronger than that of whites of comparable status. There is no reason, however, on the basis of the Gurins' work, to expect black consciousness to explain the black middle-class' sense of internal efficacy, and it does not (Table 5). Middle-class blacks feel internally efficacious politically regardless of their level of consciousness. The very type of person who can "make it" as a black in a predominantly white society must have tremendous confidence in his or her own abilities in order to get there in the first place and then again as a result of having made it. Yet, despite their confidence in their individual abilities to succeed in politics, middle-class blacks are as skeptical about the willingness and ability of government to respond to black needs as the majority of their race who are poor.

Like the poor, middle-class blacks who mistrust government and who feel internally efficacious are most active politically (Table 7). Blacks in this category participate at a rate nearly half a standard deviation above the population mean (.46), higher than the most active white group (.32). Though the interactive effect is not statistically significant for a group of this size, the findings are intriguing.

The reason cannot lie in the Gamson-Gurin thesis, which does not apply to highly motivated individuals. Higher-status blacks and whites are considerably more motivated than the poor, although for different reasons (data not shown). Higher-status whites have a stronger sense of civic duty and can more easily identify the names of political incumbents than blacks of comparable status. Middle-class blacks have stronger partisan and ideological motivation. Both have comparable levels of psychological involvement.[9]

Two alternative explanations fit the data. The first explanation, though not derived from the Gurin-Gamson thesis, is compatible with it. The second offers an important rival interpretation. On the one hand, political cynicism may be a more important motivation for all black Americans regardless of status. Black activists of all levels of income and motivation may be particularly motivated by the democratic principle of representative and responsive government which they believe to be violated in their own case. The long historical struggle to gain equal access to governmental processes has certainly made the issue exceptionally salient to the black community. Though the weak sense of external efficacy, which is intrinsic to political cynicism, would normally discourage the otherwise highly motivated from participation in politics, it is more than compensated for by the extraordinary sense of internal efficacy displayed by black activists.

On the other hand, as is so often the case with data collected at one point in time, the fact that higher-status white activists are trusting, while black and lower-status white activists are cynical, could be an indicator of the relative success or failure of those groups in the political marketplace. Black and poor activists confront a political process that favors the well-to-do and the powerful and, even at its best, is designed to respond to political majorities, not minorities. Under these conditions, efforts to gain equal access or compete politically can lead to political cynicism. In short, the simple association between trust and participation can be interpreted to mean either that trust influences political activity, or that such activity influences trust.

The issue cannot be resolved here. The rigorous types of experimental or quasi-experimental data necessary to enable us to choose among the equally plausible, but rival interpretations of the association do not exist. What data are available (Zurcher, 1969, pp. 213-33; 1970, pp. 93-94, 99-102; Shingles, 1973, pp. 251-68; 1975, pp. 49-57), quasi-experiments involving the poor in Community Action Agencies, suggests that the causality goes both ways; that those most likely to volunteer to serve on CAA boards and neighborhood committees are self-confident and politically cynical, and that membership in community action agencies can contribute to greater cynicism, depending upon the participants' level and type of involvement and the level of their initial cynicism.

Black Voter Turnout. Black nonvoting is a distinct exception to black activism. Voter registration and turnout among blacks have traditionally lagged behind those of whites in all regions of the country. By 1976, federal efforts to end discrimination in the South had brought black turnout in that region up to the same level as blacks elsewhere—11 percent below that of whites (Cavanagh, 1979, pp. 11-14). Outside the South, black voting peaked temporarily in 1964, actually exceeding that of whites of similar socioeconomic status (Verba and Nie, 1972, p. 170). Since then it has declined by 20 percent, compared to a loss for whites of only 12 percent. The greater decline in black voting occurred among the lower classes. Turnout rates for professionals and managers of both races were nearly identical in 1976 (Erie and Brown, 1979, p. 34).

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[9] Operational definitions for each of the incentives were developed from the *Participation in America* dataset: *Civic Duty to Participate in Politics:* responses to openended questions v271 and v272 (Values 40 – 49 = 1, other values = 0); *Political Information:* v266 + v267 + v275 + v276 + v277; *Psychological Involvement:* v235 + v232 + v126; *Ideological Fervor:* absolute values of the following liberal-conservative index: v139 + v141 + v143 + v147 + v329 + v240 + v293 + v294 + v295; *Partisanship:* v414 recoded to measure degree rather than types of partisanship.

Four factors have traditionally discouraged black voting: (1) economic and physical intimidation (Verba and Nie, 1972, pp. 164-71; Salmon and Van Evera, 1973); (2) fraud and legal tactics such as gerrymandering districts and holding at-large elections (Walton, 1972, pp. 33-35); (3) the fact that many black citizens do not see the relationship between the social and economic problems they face and the political process (Morris, 1975, pp. 169-78); and (4) the fact that the most visible form of government for poor blacks is the welfare bureaucracy, with the result that the officials with whom they have the greatest contact are appointed rather than elected, thereby reducing the immediate relevance of the electoral process (Erie and Brown, 1979). These factors are more likely to discourage voting among lower-status blacks than among middle-class blacks. They also help explain why blacks have increasingly turned to other forms of policy participation such as working directly with local and federal administrators in addressing community problems.

The explanation presented here to explain the over-representation of poor blacks in high-initiative policy activities provides additional insight into their under-representation at the polls. The same attitude, political cynicism, which (when coupled with a sense of internal political efficacy) encourages policy behavior among the poor, discourages allegiant behavior. Black trust in government declined between 1964 and 1972 at a rate four times that of trust among whites (ISR, 1973). If Verba and Nie (1972, p. 52) and Milbrath and Goel (1977, p. 12) are correct in classifying voting as primarily an allegiant activity, the increase in black cynicism explains *both* the decline in black voting and the increased participation of blacks in

Table 8. A Comparison of Average Standardized Scores on the Index of Voting for Respondents of Varying Combinations of the Sense of Political Efficacy and Political Trust, Controlling for Income

	Low Income[a]		Moderate-to-High Income[b]	
I. Average Standardized Voting Scores:	−.20		.10	
	Political Trust		Political Trust	
II. Average Standardized Voting Scores by Trust and Internal Efficacy:	High	Low	High	Low
Internal Political Efficacy				
High	−.27	−.10	.18	.11
Low	.06	−.42	.06	−.07
III. Test of Statistical Significance (ANOVA):	*F* Value	Sig.	*F* Value	Sig.
Main Effects	2.750	NS	8.427	***
Internal Political Efficacy	.816	NS	10.185	***
Political Trust	5.293	*	5.256	*
Interactive Effects	20.441	***	.260	NS
Total Effects	8.647	***	5.705	***
IV. Multiple Correlation, $R_{y.x_1x_2(x_1x_2)}$:[c]	.24		.25	

Source: Compiled by the author from a 1967 survey of a representative sample of Americans 21 years or older conducted by the National Opinion Research Center.

[a] Total number responding = 1097. Broken down by cell: $a = 246$; $b = 183$; $c = 232$; $d = 796$.

[b] Total number responding = 2016. Broken down by cell: $a = 320$; $b = 348$; $c = 497$; $d = 796$.

[c] y = HICPB; x_1 = sense of internal political efficacy; x_2 = political trust; and (x_1x_2) = their joint effect.

*$p < .05$
***$p < .01$
NS = not significant

policy-related activities. The data reported in Table 8 support this interpretation. Relative to the sense of internal political efficacy, political trust is a better predictor of voting among the poor than among better-motivated individuals of higher socioeconomic status (see ANOVA). The poor most likely to vote are those who have a high sense of trust and little sense of internal efficacy. This is the combination specified in generalization 6 as providing the optimal incentive for low-initiative allegiant activity. Among the poor, blacks are least likely to meet this condition.

In light of this perspective, the high black voter turnout in 1964 must be seen as an anomaly. The most likely explanation consistent with our thesis is that it was due to the short-term forces—civil rights activity and the War on Poverty—that dramatically increased the policy relevance of voting for blacks and stimulated large numbers of low-income blacks to vote for the first time (Barker and McCorry, 1976, p. 119). These primarily black issues have had less saliency in recent elections with the consequence that voting as a policy activity has had less importance to poor blacks. Thus by the mid-1970s, voting had little appeal to the black poor as either an allegiant or a policy activity.

Conclusion

The increases during the 1960s in racial consciousness, political participation, political cynicism, and self-esteem of black Americans are related. Black consciousness contributed to active efforts to influence public policy by freeing a new generation of black Americans from the mental shackles of the Protestant work ethic which imputes guilt to economically unsuccessful individuals. We use the expression "blaming the victim" to describe the practice of loading guilt on those who do not succeed in our society, a society which, throughout most of its history, has alternated between oppressing and ignoring its largest racial minority. The realization that the reason for black deprivation does not lie squarely on them has allowed many poor blacks to transfer the responsibility for poverty from themselves to society and to the most visible symbol of society, government. The result is a mentally healthier and politically more active black citizenry. The strong sense of self-confidence and a deep suspicion regarding the willingness and ability of government to respond to their needs has proved to be an important catalyst, mobilizing blacks to seek to influence the policy process.

The federal government has had an important, if unintended, role in shaping the nature of black political participation. It is second only to the civil rights movement in spurring the development of black consciousness. By declaring war on both poverty and racial discrimination in the 1960s, the federal government legitimized and politicized black goals of social and economic equality. Needs changed to political demands as the idea set in that society has a responsibility for black deprivation and as the government assumed increasingly greater responsibility for black welfare. Both at the symbolic level of political rhetoric and the operational level of the local welfare office, it has been acknowledged that government deserves the credit or, more likely, the blame for the fortunes of black people.

It is paradoxical that both the government's apparent retreat from its initial commitment to black goals and the continued growth of a welfare bureaucracy have contributed to a deepening mistrust of government among blacks, the former because of the image of betrayal it created, the latter because it symbolizes the moral responsibility of government for the deprivation of the black poor. Equally paradoxical, the decline in trust among blacks has contributed to a further decline in black voting at the same time that it has encouraged high-initiative policy behavior.

An understanding of differing bases of blacks' and whites' participation helps to clarify their political relation. Blacks make a greater effort to influence public policy than poor whites who, having no equivalent ideology, are more inclined to accept the implications of the Protestant work ethic for their sense of self-worth and the utility of political action (Verba and Schlozman, 1977; Lamb, 1975, pp. 61-78). Blacks therefore more often speak for all of the poor even though the overwhelming majority of those living in poverty are white.

Among poor activists, blacks focus more than whites on the policy process and less on purely allegiant activities. In their efforts to influence policy, they are likely to convey a distrust for government officials with whom they interact. Therein lies the potential for conflict. However, the conflict does not necessitate nor does it usually involve confrontation politics. The black violence and the nonviolent direct action of the 1960s have received too much attention. In its fascination with the dramatic and the unusual, the scholarly community (like the press) has overlooked the fact that black Americans are heavily involved in conventional politics. The tactics they employ are, for the most part, no different from those used historically by other ethnic groups who worked through the political process and in their communities in seeking an equitable place in American society.

Although this discussion has been necessarily confined to black politics, it has broad relevance for other minorities. The ability of black consciousness to mobilize black Americans supports Nelson's position that ethnicity rivals socioeconomic status as the principal analytical tool for understanding political participation (1979). Comparable ideologies like "brown consciousness" and "red consciousness" may have a similar importance for Hispanic and Native Americans (Welch et al., 1975; Greenberg, 1974, pp. 131-72). The key to policy-relevant behavior, however, will not be ethnic identity per se, but the existence of a counterculture which politicizes the community by having its members transfer responsibility for their deprivation from themselves to the political system.

Finally, the discussion has relevance beyond specific groups. It contributes to our understanding of the complex relationship among sense of political efficacy, political trust and political participation. There are multiple relationships among them, depending upon which components of the variables are involved. The specification of these components, the relationships among them and the conditions under which these relationships hold should constitute a constructive step in the development of a more general theory of political participation.

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The Sources of Political Tolerance: A Multivariate Analysis

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Over the past 25 years a number of conclusions concerning the development of political tolerance have come to be well accepted in the literature on political behavior. There are, however, two persisting problems with the studies that have generated these findings: they have relied on a content-biased measure of tolerance, and have failed to examine well specified models of the factors leading to tolerance. In this article we report the results of an analysis of the determinants of political tolerance using a content-controlled measure of tolerance and a more fully specified multivariate model. The parameters of the model are estimated from a national sample of the U.S. The results indicate the explanatory power of two political variables, the level of perceived threat and the commitment to general norms, and psychological sources of political tolerance. Social and demographic factors are found to have no direct effect and little indirect influence on the development of political tolerance.

The study of political tolerance has been a major component of public opinion research in the 25 years since Stouffer's seminal contribution (1955). Over this time, a number of findings on the determinants of tolerance have become accepted. Yet there are two reasons to suspect the general conclusions of these studies. First, as we have attempted to show earlier (Sullivan, Piereson and Marcus, 1979), previous studies of political tolerance were flawed in their measurement of the dependent variable. The use of items developed by Stouffer, all with left-wing targets (communists, socialists, and atheists), creates problems that will affect the distribution of political tolerance and will bias estimates of relationships between tolerance and other variables. The content bias resulting from using only left-wing targets means that respondents who are intolerant of *other* groups may erroneously be classified as generally tolerant. Such measures confound the willingness to extend rights to objectionable groups (tolerance) with indifference or perhaps even support of the groups specified. Correlations involving these measures of tolerance are thus difficult to interpret since they invariably tap attitudes toward the group in question as well as the construct of tolerance. We have shown elsewhere the distortion that results when the selection of a target group is related to socioeconomic characteristics such as education and urban/rural residence (Sullivan, Marcus, Piereson and Feldman, 1979; and Marcus, Piereson, and Sullivan, 1980).

The second problem with previous studies of political tolerance has been their reliance on simple correlations to establish the relationships between tolerance and a number of independent variables. Bivariate correlations, however, can be misleading when other important factors influence both variables in question. This sort of specification error can affect the magnitude and even the direction of parameter estimates. Yet, so far as we have been able to find, no study presents or evaluates a well-specified model of political tolerance. As a result, many currently accepted findings on the determinants of political tolerance may be based on severely biased estimates of the relationships between key independent variables and tolerance.

We report here an analysis of the determinants of political tolerance that attempts to correct these problems. We use a content-controlled measure of tolerance to remedy the content-bias of previous measures. This measure is used in a multivariate analysis to evaluate a more fully specified model of political tolerance. In addition, we rely

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on maximum likelihood confirmatory factor analysis (LISREL) to fit the model to our data. The advantage of this approach to model fitting is that it incorporates a measurement model into the data analysis. We are thus able to examine the reliability and appropriateness of our indicators as one part of the overall evaluation of the model. This will further raise confidence in our results since parameter estimates will be free of the effects of measurement error and the major constructs will be more clearly defined.

Political Tolerance and Democratic Theory

Much of the theoretical significance of political tolerance derives from its role in varieties of democratic theory. Its importance depends on two sets of empirical questions. The first of these questions involves the distribution of levels of tolerance in society and the relationship of tolerance to political activism. Given the hierarchy of political roles available to citizens today, from those uninvolved in politics to those in formal positions of authority, the citizen's opportunity (and thus responsibility) to sustain the "rules of the game," including ensuring political access to potentially objectionable groups, may vary across these roles in a systematic way. Empirical findings suggesting that those with greatest responsibility have greater commitment to the norms and maintenance of political tolerance would support the continuing hierarchy of influence among political roles as necessary to sustain the tolerant character of the regime. Alternatively, finding the aggregate level of political tolerance to be fairly uniform across different political roles would support a more equitable redistribution of political influence. Thus how tolerant various classes of people are in various political roles has been one of the abiding areas of dispute in the contention between "elitist or revisionist" theories of democracy and "participatory" theories of democracy (see Walker, 1966; Pateman, 1970; Thompson, 1970).

A second important way in which the empirical study of political tolerance can affect views on democratic theory lies in the question of the relationship of political tolerance to other factors. As societies change, for example by becoming more educated, more industrialized and more urban, these changes may alter the extent and nature of tolerance in the political culture. We need, therefore, to explore the determinants of political tolerance and how changes in these determinants are likely to affect the level and distribution of political tolerance. For example, while finding low levels of political tolerance, Stouffer (1955) also found powerful links between tolerance and education which he expected would lead to increased tolerance in the future. The extent to which political tolerance is linked to determinants likely to go through progressive changes that will in turn ameliorate political intolerance is thus an important empirical question. The answer will add fuel to the controversies as to which type of democratic theory is best suited to our values and circumstances (see Pennock, 1979; Thompson, 1970). Some varieties of democratic theory, for example, have strong developmental expectations. According to such theorists as J. S. Mill, the political culture and the political judgment of the electorate can be expected to improve as formal education and practical political experience become more widespread throughout the public. A model of the development of political tolerance can help to assess the empirical status of such theories.

Development of the Model

We will consider variables taken from two sources. First, we will review the major independent variables that previous studies have shown to have significant relationships with political tolerance. Such variables fall into three broad categories: social (or demographic) characteristics such as education, occupation, age and religion; psychological (or personality) characteristics such as self-esteem, authoritarianism and dogmatism; and political characteristics such as political ideology, political involvement and commitment to democratic norms. Second, we will consider those factors that are theoretically important even if research has not yet demonstrated significant relationships.

The Dependent Variable: Political Tolerance

As we noted in our previous work (Sullivan, Piereson and Marcus, 1979) a measurement procedure is needed which allows respondents themselves to specify the groups they most strongly oppose. In an attempt to obtain such a measure, we developed and tested the following measurement approach on a national survey of 1509 respondents conducted for us by the National Opinion Research Center in the spring of 1978. First, we provided each interview respondent with a list of potentially unpopular groups that ranged from communists and socialists on the left, to fascists, John Birch Society members, and Ku Klux Klan members on the right. We also included a number of groups, such as atheists, pro-abortionists, and anti-abortionists, which we expected in some ways to represent positions that are independent of the left/right dimension. Respondents were then asked to identify the group they liked the least, and we made it very clear that they could also select a group not on the list. Respondents were then presented with a series of statements in an agree/disagree format which elicited their views about a range of activities in which members of that group might have participated. The following statements were among those included in the series:

1. Members of the ______ should be banned from being president of the United States.
2. Members of the ______ should be allowed to teach in the public schools.
3. The ______ should be outlawed.
4. Members of the ______ should be allowed to make a speech in this city.
5. The ______ should have their phones tapped by our government.
6. The ______ should be allowed to hold public rallies in our city.

The statements were read as they appear above with the blanks filled with the group selected by each respondent.

Our intention was to avoid contaminating the tolerance/intolerance dimension with the respondents' political beliefs. If we had merely asked all respondents whether communists should be allowed to hold public office, their responses would have depended not only on their levels of tolerance, but also on their feelings toward communists. The advantage of our procedure is that it creates a situation in which the evaluation of each respondent toward the group in question is held constant. Clearly, our measures are not "content-free" since there is a context and a specific group toward which each respondent must react. We thus call it a "content-controlled" measure, to emphasize that we have attempted to "control for" the content by allowing respondents to select functionally equivalent groups.

In the following analysis, we rely upon a six-item political tolerance scale, based on the six political tolerance items reviewed above. Since each item has five-point agree/disagree response categories, scores on the six-item scale range from 6 to 30. The mean score for the six-item scale is 16.1, well below the midpoint of 18; and the coefficient alpha, an estimate of the lower bound of reliability, is .78.

Social Determinants of Tolerance. Since Stouffer's in-depth empirical study of tolerance, a number of basic demographic variables have been thought to influence political tolerance. Foremost among these is education. Education is thought to increase familiarity with diverse ideas and people. Moreover, the citizen must *learn* that a free market of ideas is vital to American democracy and that nonconformists are not necessarily bad. This is basically a cognitive explanation; one learns the principle that free exchange of ideas is necessary and that to be different is not necessarily to be bad and dangerous. Thus education plays a dual role in increasing levels of information and increasing the willingness to accept hitherto threatening information.

While this argument makes no attempt to tie education into the more general aspects of the social structure, high social status is also likely to be linked to increased experience (hence acceptance of diversity of opinion) and increased security from threat (hence greater willingness to allow others to hold potentially threatening ideas and to display potentially threatening behavior). This reasoning would lead us to expect social status to be related to tolerance in somewhat the same way as education is (see Korman, 1975).

In our study, we examine the impact of both education and social status, the latter measured by income and occupational position. Social status is measured by the Hodge-Siegel Prestige Score, which is assigned to the reported occupation of the respondent (or in the case of the unemployed, of the respondent's spouse; for details see Siegel, 1971).

Previous researchers have found a relationship between age and political tolerance. Because younger respondents are likely to be more educated, and because the younger cohorts of respondents have experienced a political climate that should be more liberalizing and enlightening than that of their elders (a period effect), age should be inversely related to tolerance. The increasing commitment to civil rights for blacks, women and the poor, particularly on the part of the young in the 1960s, may have given the younger respondents a greater commitment to political tolerance, or so the argument runs. Thus, while the link between age and tolerance may be strongly affected by the influence of education (Stouffer, 1955; Nunn et al., 1978; Cutler and Kaufman, 1975), it appears from other studies that even after the influence of education is removed, age may still have a conservative effect on political tolerance.

Past studies (Stouffer, 1955; Nunn, et al., 1978) have also uncovered what appears to be a relationship between religion and tolerance of ideological nonconformity. Following Stouffer, Nunn and his associates found that in 1973, Jews and non-religious people were the most tolerant, followed by Catholics and Protestants, with little difference between the latter two religious groups. The percentage differences discovered were substantial, as approximately 46 percent of Protestants, 59 percent of Catholics, 88 percent of Jews, and 87 percent of non-religious people were categorized as "more tolerant" on Nunn's scale.

Other researchers have discovered the relationships between tolerance and additional social variables such as sex, size of city, and region. Our research, reported elsewhere (Sullivan et al., 1982) shows that all of these social background variables relate with target-group selection in such a way that the content-bias of the traditional measurement procedures may have produced misleading conclusions about the correlates and determinants of political tolerance. For example, the following respondents are most likely to select right-wing targets: those with more education, younger respondents, Jewish or non-religious respondents, urban residents, residents of the East, and so on. On the other hand, the following respondents are mostly likely to select left-wing targets, precisely those groups included in previous studies of tolerance: the uneducated, those of the older generation, Catholics or Protestants, rural residents, residents of the South and Midwest, and so on. Thus previous studies may have "stacked the deck" by asking about groups which are, in some sense, harder for certain respondents to tolerate.

Psychological Determinants of Tolerance. In addition to the studies which have focused on the demographic determinants of political tolerance, other studies have examined mainly the psychological and personality sources of political tolerance (Knutson, 1972; Zellman and Sears, 1971; Zalkind, Gaugler and Schwartz, 1975). The most consistent finding links self-esteem (or a closely related concept) to political tolerance. Most analysts have examined self-esteem within a trait conceptualization of personality, although Sniderman (1975) viewed low self-esteem as a hindrance to the social learning of political norms, including tolerance. Sniderman argued that low self-esteem leads to intolerance because it interferes with social learning. Although the norms of society may be tolerant ones, a significant portion of the public will neither learn nor adhere to these norms because this requires a facility for abstract thought that many do not possess. In particular, people with low self-esteem will "reject the norms of democratic politics not because they are motivated to do so but largely because their negative self-attitudes have impeded the learning of these values" (p. 178). While he concentrates on the social learning argument, Sniderman also argues that low self-esteem may interfere with the motivation to learn norms and ideals such as tolerance.

Sniderman used three different measures of self-esteem that correlate highly with one another and produce similar results. In the interests of parsimony, we selected his eight-item personality unworthiness scale as the measure of self-esteem we would use in our multivariate analysis. The scale has a reliability (coefficient alpha) of .69.

Probably the most dominant tradition in political psychology is research into the authoritarian personality (see Sanford, 1973). In essence, the authoritarian personality construct is based on a trait approach—an approach which assumes personality to be a collection of traits within the individual. Authoritarianism consists of a number of important characteristics including authoritarian submission, defined as a basic need to obey those in authority and to command subordinates; aggression toward outgroups, which means redirecting underlying hostility of authorities toward weaker scapegoats; stereotyped thinking—that is, intolerance of ambiguity and a strong tendency to think in terms of black and white categories—and other similar characteristics. Seen in these terms, the relationship of authoritarianism to political intolerance is probably close, although tolerance is an attitude rather than a personality trait and the two must be separated analytically.

A serious problem, from our point of view, are the criticisms that the traditional measure of authoritarianism, the F-scale, is appropriate to measure authoritarianism on the political right but not on the left. Theoretically, there is no reason why left-wing belief systems cannot be rigid or authoritarian. One major alternative to the F-scale is Rokeach's work on dogmatism (1960) which attempts to measure the degree of people's open-mindedness and flexibility without regard to ideological content. We include seven dogmatism items which create a scale with a coefficient alpha of .77.

Another strategy is to employ an indirect measure that taps a central aspect of the authoritarian syndrome. To accomplish this, we have included three items from Rosenberg's faith in people scale (1956) and two items from Martin's and Westie's threat orientation scale (1959). Since an important characteristic of the authoritarian personality is distrust of other people, their motives and their impulses, we deem the five items an appropriate, if simple, measure of the affective component of authoritarianism. These items create a scale with a coefficient alpha of .69.

Another major approach is that of Knutson (1972) who relies on the conceptualization of personality developed by Maslow. In this scheme, human personality is dependent on the satisfaction of various needs, ranging from the most basic (physiological and safety needs) to the more complex (affiliation, self-esteem, and self-actualization needs). These needs form a definite hierarchy with the more basic needs requiring satisfaction before the needs on the next level of the hierarchy can be met. Knutson has characterized these as lying along a continuum with "concern with self" at one end and "concern with self in relation to one's environment" at the other. The self-centered person is unlikely to experience empathy; since most of that person's energies are directed toward meeting personal, basic needs, abstract ideas such as tolerance are unlikely to receive much attention from such a person. Thus, at one end, individuals are expected to be self-centered and most of their behavior should be directed at immediate, concrete goal fulfillment. At the other end, individuals are expected to consider and examine broader perspectives and to be concerned with how they relate to others and to the social environment more generally. Using Knutson's formulation, we would anticipate a monotonic relationship with higher levels on the need hierarchy linked to greater tolerance.

In order to assess people's positions on Maslow's need hierarchy, we asked people in our national study to select their most important value from a list provided them. We selected one value that represented each level on the need hierarchy (see Inglehart, 1977; Marsh, 1977). The values are listed in Table 1 along with the need level they purport to measure and the percentage of our national sample selecting each as most important. The relationship is generally monotonic. There is a strong increase in political tolerance as one moves from the safety-security need level to the affiliation need level.

To anticipate our findings somewhat, we did not find that these four psychological variables were able to discriminate among the various approaches to personality and political tolerance. Our analysis showed all to be linked to political tolerance, and furthermore, that the most powerful analysis was to incorporate the four indicators as alternative measures (multiple indicators) of a single concept: personality. Since the four indicators are so highly intercorrelated and the measurement approach we use corrects correlations for attenuation due to unreliability, multicollinearity problems require us to use them as multiple indicators. Conceptually then, we combine the four personality variables in a single factor, perhaps best defined as "psychological security." The four indicators have much in common because each focuses, in importantly different ways, on the ability of the individual to confront reality and social experience, and to deal with a world that is complex, ambiguous and threatening. The variance shared by these indicators suggests that people vary in the extent to which they are psychologically prepared to engage a socially and politically complex environment.[1]

Political Determinants of Tolerance. The most serious omission in previous studies of political tolerance has been the failure to examine *political* tolerance as an inherently *political* concept with its source in *political* processes. We will attempt to correct this deficiency by examining the importance of three sets of political variables, including political ideology, political threat, and support for the general norms of democracy.

The differences between liberals and conservatives are sometimes thought to account for various degrees of political toleration. At times, the terms "tolerance" and "liberalism" are used in-

Table 1. Respondents' Choice of Most Important Need

Maslow Need Hierarchy	Value	Percent of Sample	Mean Tolerance Score*
1. Physiological Needs	Comfortable Life	24	14.9
2. Safety and Security Needs	Security	31	15.0
3. Affiliation and Love Needs	Affection	19	16.7
4. Esteem Needs	Esteem	20	17.7
5. Self-Actualization Needs	Originality	6	18.0
N = 1509			

Source: Computed from 1978 Political Tolerance Survey conducted by the National Opinion Research Center, University of Chicago.

*On the six-item scale which ranges from 6-30.

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[1] The excessive intercorrelations of these personality measures prevents any direct comparisons among the various psychological explanations. Such a result is not totally unexpected; unfortunately it may be predicted from several theoretical perspectives. Knutson (1972) demonstrates that the need hierarchy formulation can account for the development of a number of personality characteristics, including the ones used here. The data are also consistent with Rokeach's argument (1960) that low self-esteem will lead people to develop dogmatic belief systems and to exhibit little faith in other people. These results are consistent with the broad outlines of the theory of the authoritarian personality.

terchangeably so that the acceptance of norms of toleration is said to be a liberal position and the rejection of tolerance, or a more guarded acceptance, is said to be a conservative or "illiberal" position (see Lipset and Raab, 1970, pp. 432-33). Those who make this connection, however, are usually careful to distinguish between economic and non-economic issues, where the former refers to questions of the distribution of wealth and the latter to those of cultural conformity and nonconformity. In this sense, tolerance is understood to be part of the social or non-economic dimension of domestic liberalism. Tolerance is thus associated with issues such as the legalization of drugs, acceptance of cultural nonconformity and opposition to traditional women's roles.

From another perspective, social scientists such as McClosky (1960) and Eysenck (1954) have suggested that ideological identification is linked to personality characteristics that might lead us to anticipate a link with tolerance. Specifically, McClosky argues that support of certain conservative beliefs signals a personality unlikely to engage in political discourse, to adapt to social change, to extend universal norms to strangers, and to engage in stereotyping and discriminations. Thus this non-economic dimension of ideology may relate to tolerance in part because of certain underlying personality characteristics. While previous studies of political tolerance may have overestimated the impact of ideology due to an inherent left-wing bias, we advance the tentative hypothesis that those on the left will be more tolerant of extremist groups on the right than those on the right will be tolerant of corresponding groups on the left.

Two measures of ideology, in the left/right sense, have been adopted. The first is an index of ideological self-placement, a seven-point liberal/conservative scale on which respondents were asked to locate themselves. The seond measure is based on the ideology of respondents' two least-liked groups. As we demonstrated earlier (Sullivan, Piereson, and Marcus, 1979), respondents at the liberal end of the spectrum were most likely to select targets from the right, and vice versa. It is therefore possible to contrive a measure of ideology based on the target groups selected. Thus, those who chose two right-wing groups were classified as the most liberal, while those who chose two left-wing groups were classified, on a three-point scale, as the most conservative. Those who picked one group from each end of the spectrum were placed in the middle of the scale.

Previous studies have also shown that the level of intolerance in individuals is directly related to their perceptions of dissident groups as posing a threat (see Stouffer, 1955, Ch. 8). Thus intolerance arises from people's perceptions that dissident groups threaten values important to them or constitute a danger to the constitutional order. Yet it is also true that many people who perceive dissident groups as threatening are nevertheless prepared to tolerate them and to defend their procedural claims. It is appropriate, therefore, to test this general proposition within a multivariate framework with our data.

We attempted to measure the threat posed by each respondent's least-liked group by presenting a series of semantic differential adjectives about the group in question. We began with eight adjective pairs, which were factor-analyzed. The five adjective pairs with the highest loadings on the first factor extracted were selected for use as indicators of perceived threat: honest/dishonest; trustworthy/untrustworthy; safe/dangerous; nonviolent/violent; and good/bad.

While previous studies (Prothro and Grigg, 1960; McClosky, 1964) have concluded there is little relationship in the mass public between support for some general democratic principles and the specific applications of these principles, this conclusion—based on a comparison of marginal distributions—needs more examination. The observed consensus in favor of the abstract statements and the lack of any such consensus on the specific items were taken to mean that there was no relationship between the two. A more appropriate way to approach the problem is to measure the relationship between the degree to which individuals support the abstract principles and the degree to which they apply them in practice.

To examine this question, we repeated in our survey two of the questions used by Prothro and Grigg and four used by McClosky. These items, along with the distribution of responses in our survey, are summarized in Table 2. The first four items express democratic principles in highly abstract form, while the last two are more specific and hence more controversial. The distribution of responses to these items is very close to those reported in the earlier studies, despite Prothro's and Grigg's use of local samples, and the passage of nearly 20 years. It is still true that the overwhelming proportion of the American public supports the principles of minority rights, majority rule, equality under the law, and free speech when these principles are posed in an abstract form, as they are in the first four items of the table.

Although the marginal distributions of responses to questions ascertaining support for abstract principles and to questions ascertaining support for specific situations of political tolerance may vary substantially, it does *not* follow that there is no relationship between the two categories of questions. Among recent writers, only Lawrence (1976) has been sensitive to this problem as he tried to measure the relationship between respondents' positions on the general norms regarding tolerance and their willingness to apply them in practice. Using different items, Lawrence found considerable consistency between support for general norms and their application to specific circumstances, concluding: "Large majorities of the population in fact apply their tolerant general norms consistently on even the hardest . . . issues" (p. 93).

Although the proposition is often taken for granted that abstract beliefs influence responses to practical situations, it is not surprising to find that there is not a one-to-one relationship between the two. Competing values usually operate in practical circumstances, so that people are forced to find some compromise between or among them. Hence, we hypothesize that the stronger their commitment to the general norms of democracy, the more willing people should be to act on those norms. Table 2 presents the simple correlations between the various questions measuring support for the general norms and our six-item tolerance scale, which is based on responses to specific situations and political groups. These correlations, while not particularly high, are all in the expected direction. In interpreting these correlations, readers should bear in mind that there is little variation in the responses to these four items (note the distributions in the table), a circumstance that will usually reduce the size of the correlations.

In addition to the variables of left/right ideology, perceptions of threat and support for general norms concerning tolerance, additional political variables such as political interest, participation, and information were also examined. There is sufficient theoretical reason to expect a strong relationship between an individual's political involvement, for example, and political tolerance. Participatory democracy theorists (such as Pateman, 1970) have argued that the process of political participation is an educative one, and that tolerance is one of the natural outcomes of such a process. Empirical studies have found large differences in tolerance between the participatory elite and the non-participatory masses (cf. Stouffer, 1955, p. 57). We find that most of these differences are probably the result of the content-bias

Table 2. Support for General Principles of Civil Liberties (Percent)

General Statement[a]	Agree	Uncertain	Disagree	Agree with Prothro-Grigg or McClosky[b]	Correlation[c] with Six-Item Tolerance Scale (Specific Acts)
1. People in the minority should be free to try to win majority support for their opinions.	89	9	2	94-98	.25
2. Public officials should be chosen by majority vote.	95	3	2	94-98	.27
3. No matter what a person's political beliefs are, he is entitled to the same legal rights and protections as anyone else.	93	4	3	94	.22
4. I believe in free speech for all no matter what their views might be.	85	7	9	89	.29
5. When the country is in great danger we may have to force people to testify against themselves even if it violates their rights.	35	16	48	36	–.14
6. Any person who hides behind the laws when he is questioned about his activities doesn't deserve much consideration.	52	16	32	76	–.23
N = 1509					

Source: Computed from 1978 Political Tolerance Survey, conducted by National Opinion Research Center, University of Chicago.

[a] The first two questions listed are taken from the Prothro-Grigg (1960) questionnaire, while the last four are taken from McClosky's (1964). Our questions were presented in the form of five-point agree-disagree scales. The agree column reports the percentage that agree or strongly agree while the disagree column reports the percentage that disagree or strongly disagree. In the original studies, these questions were presented dichotomously so that respondents either had to agree or disagree.

[b] Reported in Prothro and Grigg (1960) and McClosky (1964).

[c] Correlations are Pearson's r.

of the Stouffer-based questions, because in our national sample we find that political participation relates strongly to target group selection, but not to tolerance (the higher participants dislike right-wing groups more). The parameter estimates in our model are unaffected by excluding these additional political variables.

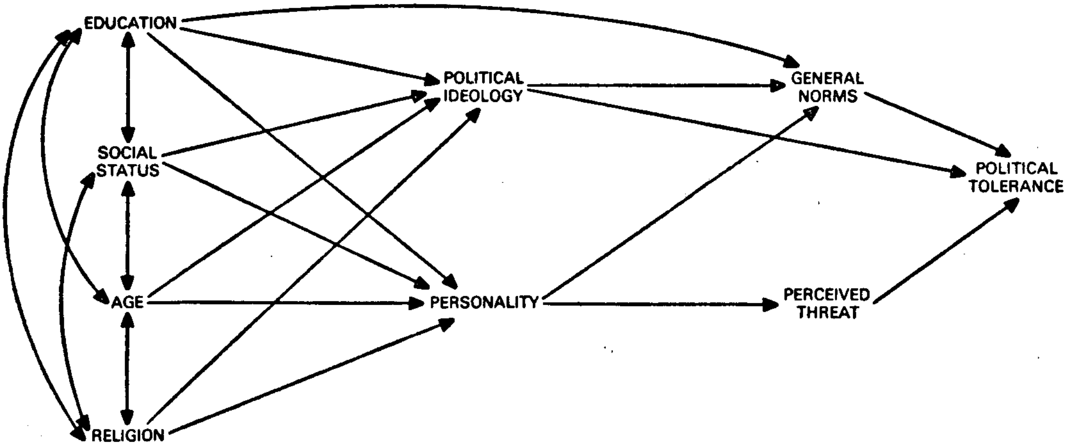
Multivariate Analysis of Political Tolerance

In this section, we specify a more complete model of political tolerance including variables from each of the three sets. In general, we assume that social variables are causally prior to psychological variables, which in turn precede political variables. Certainly these are dynamic relationships. As our analysis uses a cross-sectional survey, it must necessarily be a static one. We assume that this general causal ordering represents the major processes underlying the relationships among these variables.

Figure 1 presents the hypothesized relationships among endogenous and exogenous variables in our model of political tolerance. We assume the most important immediate causes of tolerance to be twofold: adherence to the general norms of civil liberties, and the perceived threat posed by the group toward which the tolerance (or intolerance) is directed. The reasons for our expectations are outlined above, and there is no compelling reason to expect that the impact of these two variables would be mediated by any of the other endogenous variables.

An individual's perception of another group as threatening is undoubtedly affected by psychological processes rather than resulting solely from realistic assessments of the danger of a particular disliked group. Some respondents may be psychologically predisposed to perceive threats whether they exist or not; and, thus, some people may be predisposed to be intolerant while others may not. The psychological characteristics discussed earlier, such as dogmatism and low self-esteem, might therefore explain perceptions of threat and, indirectly, tolerance and intolerance. If this argument were completely true, then levels of tolerance and intolerance in the society would be independent of actual political developments. We take the view here that perceptions of threat do involve evaluations of the political strength of, and the danger posed by, dissident groups, although since such perceptions are subjective, they will be affected by psychologial variables such as personality.

Turning to political ideology, we have already noted the liberal sources of tolerance. There is considerable reason to expect liberals to be more tolerant than conservatives. We fully expect some of this impact to operate through support for general norms of democracy, since the norms of democracy are themselves liberal norms. Thus liberals should be more likely to adopt these norms than are conservatives. There should also be some impact of ideology which operates directly on political tolerance, because while not all rank-and-file liberals and conservatives can be expected to comprehend and adopt the general norms most consis-



Source: Compiled by the authors.

Figure 1. Fully Specified Model of Political Tolerance

tent with their ideology, they might well be expected to adopt the more concrete implications of these norms.[3] However, there is no reason to expect that people's ideologies would relate directly to what they perceive as threats, since there are ample threatening targets in the eyes of both liberals and conservatives.

The role of personality as to its influence on political tolerance has been discussed above. It is unclear whether the impact of personality on tolerance should be expected to be direct, or to be merely indirect, through its link to perceptions of threat and support for toleration norms. Some of the impact of psychological factors is no doubt indirect, as we noted in its role in the development of the perceptions of threat from other groups and ideas. In adddition, those respondents who are more dogmatic, low on Maslow's need hierarchy, low in self-esteem and low in trust in people, can be expected to be more conservative than their opposite counterparts, and to be less likely to learn accurately the norms of the political system, leading us to expect a relationship between inner security and the support for general norms of democracy (Sniderman, 1975).

It is less clear what to expect regarding the direct impact of personality on political tolerance. Certainly, Sniderman's analysis (1975) suggests a direct impact, but it is plausible to explain this as working through the impact of personality on the learning of general norms. Perhaps those with low self-esteem are less tolerant merely because their lack of esteem interferes with learning general norms of democracy so that they cannot therefore apply them in specific situations. On the other hand, in addition to affecting the learning of general norms of democracy, low self-esteem may enhance the tendency to project personal inadequacies onto hated scapegoats, as suggested by the traditional literature on personality and politics. This could operate above and beyond this indirect effect, and in a sense, a comparison of the direct and indirect effects of personality on tolerance may provide a crude test of the social learning hypothesis as against the more traditional personality-trait hypothesis. The indirect effect of personality on toleration through support for general norms of democracy may represent the effect of personality through social learning, while the direct effect may represent its effect through traditional personality traits such as authoritarianism.

The four sociological variables in our model (all exogenous) include education, social status, age, and religion. Education is expected to have a major direct impact on three variables: political ideology, personality, and understanding general norms. Education has been shown to be related to tolerance in many studies, reviewed above, but these studies have not attempted to discern whether its impact is mediated by other variables. Furthermore, our evidence suggests that education relates more strongly with the selection of target groups than with tolerance per se (Sullivan et al., 1982).

There is good reason to expect education to have an impact on at least three endogenous variables other than political tolerance. It ought to have two opposite impacts on ideology. Previous studies have shown that education leads to greater economic conservatism, and greater social liberalism (Erikson and Luttbeg, 1973; Ladd, 1978). Thus the composition of our political ideology variable—social rather than economic—determines the direction of the relationship between education and ideology. Education also ought to affect personality—particularly those aspects of it that we have discussed here—self-actualization, dogmatism, and self-esteem. Persons with higher levels of education ought to be more competent in their environment, generally better able to manipulate and understand the forces which affect them, and thus better able to close the gap between the ideal self and the perceived self, i.e., to have higher self-esteem. They should also learn about diverse points of view and ways of experiencing life, thus becoming less dogmatic, even if they had a tendency toward closed-mindedness before achieving a high level of education. Finally, education ought to enable people to learn better the general norms of democracy. Since these norms are abstract, and require the ability to understand concepts, the highly educated should be more adept at learning and understanding them. Indeed, this was one of the major findings of both Prothro and Grigg (1960) and of McClosky (1964). The major unanswered question about the impact of education is similar to the question regarding the impact of personality: does it have any direct effect on political tolerance, above and beyond its impact on personality and support for general norms?

Social status should have an impact similar to that of education, while age should affect two of the political variables directly—older respondents should be more conservative (both on social and economic issues), and they should be more dogmatic and have lower self-esteem (see Erikson and Luttbeg, 1973). Religion is defined as secular detacchment. We have found that specific religious preference is related to target group selection, rather than political tolerance.[3] Secular detachment should have basically the same impact as age.

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[3] Although in the United States we fully expect even most conservatives to adopt the norm of tolerance, at least in the abstract, we do believe that conservatives will be more likely than liberals to trade off tolerance against other political values, such as stability and security. It is, of course, a matter of degree.

The model in Figure 1 presents the variables that we expect to have direct and significant impacts on one another. It is not a fully recursive model because several possible recursive relationships are not expected to be significant and have therefore been deleted. In estimating the parameters of the model, we estimate a fully recursive model, although generally we will report only those coefficients which are statistically significant. In this way, our a priori expectations can be compared with the modeling results; it will become clear which of the hypothesized direct paths, in Figure 1, are erroneous and should not have been specified, and it will likewise be clear which unspecified paths do in fact appear to exist between variables.

Parameter Estimation

We rely on maximum-likelihood confirmatory factor analysis (LISREL) to fit the model to our data. The problem of measurement error is addressed by allowing the researcher to specify first a set of theoretical relationships among unmeasured, theoretical constructs; and second, to specify a set of measurement relationships between these theoretical constructs and their empirical indicators. Using only information about the relationships among indicators, the researcher is able to obtain estimates of the relationships among the theoretical constructs (called *structural parameters*) and estimates of the relationships between each theoretical construct and its empirical indicators (called *epistemic correlations*).[4] The details of these statistical proccedures are presented in Jöreskog (1969, 1970, 1973); a brief summary may be found in the appendix. Most of the theoretical constructs are measured by several empirical indicators. The epistemic correlations help both to determing which indicators are the most reliable measures of each construct, and to assess the "true nature" of the construct.[5] Table 3 presents the epistemic correlations for those constructs with multiple indicators and for the six-item tolerance scale.[6] Single-indicator exogenous variables are not presented in this table.

The epistemic correlations are consistent with the definitions given to the constructs. Interestingly, the general norms construct seems primarily defined by the political process rules (majority vote and free speech) rather than by legal rules. The single value-actualization item relates only weakly to personality, and the common semantic differential format of the perceived threat indicators introduces some minor correlated error (-.13 between honest/dishonest and safe/dangerous).

Regarding the personality construct, the dogmatism scale is the "best" indicator. It practically defines the construct, although the self-esteem scale also has a large epistemic correlation. The other two variables—the value measure of self-actualization and the faith in people scale—are of lesser importance than dogmatism and self-esteem in defining the personality construct. Thus, although we label the construct psychological security, it is closely related to the traditional dogmatism-authoritarianism syndrome presented in the personality and politics literature, and in personality research generally. A lack of self-esteem is often hypothesized to underlie this syndrome, and the epistemic correlations suggest that this is the case here. The dogmatism scale also has the highest bivariate correlation with political tolerance.

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[3] We divided our sample into the same four religious groups: Protestants, Catholic, Jewish, and non-religious respondents. Using our content controlled measure we found almost no difference among Protestants, Catholics and Jews in their levels of political tolerance, although we did find some large differences in percentages selecting left-wing groups as least-liked targets. This again suggests the importance of the content bias of the original Stouffer questions, as Jews are more tolerant of left-wing groups but not of right-wing groups. We find that people who adhere to no particular religious faith are more tolerant than the more religious respondents, and in fact the differences are large. The only difference in tolerance scores among the various Protestant denominations is that Baptists are less tolerant than the others. To maximize the relationship with tolerance, we coded Baptists 1, non-religious people 3, and all others 2. When this is done, the correlation with our tolerance index is .26. Thus, although religion appears to be an important variable in understanding political tolerance, it is primarily a secular detachment from religion that is important, not whether one is Jewish, Catholic, or Protestant.

[4] Since almost all of the variables in this analysis are attitude or personality scales which have arbitrary units of analysis—and since the major concern is a comparison of the relative impact of different variables—we have relied on standardized rather than unstandardized coefficients.

[5] Readers versed in factor analysis, can interpret the epistemic correlations as factor loadings and the abstract constructs as factors. The pattern of loadings helps to label and interpret the factors.

[6] The epistemic correlation between the tolerance scale and the underlying construct is obtained by taking the square root of the scale's estimated reliability. Coefficient alpha for the tolerance scale is .79, so the epistemic correlation is fixed at .89. The other epistemic correlations are estimated using the excess information, provided by the over-identified model, using the LISREL program. In the case of the personality construct it will not generally be true that the epistemic correlations equal the square root of the estimated reliabilities. This is the case because the epistemic correlations reflect both the reliability of the appropriate scale and its relationship to the more general personality construct.

The relationship between the theoretical constructs are called structural parameters (*partial* path coefficients, corrected for measurement error). The structural parameters represent the strength of the relationships between each of the theoretical constructs included in the model, controlling for the other constructs. Figure 2 presents the political tolerance model with the exogenous and endogenous variables included. As we previously noted, political involvement is not included inasmuch as no direct or indirect relationships with political tolerance were found.

The perception of others as posing a threat has the strongest direct impact on political tolerance; the coefficient of -.43 shows that the more respondents perceived a threat from the target group the less their political tolerance. The general norms construct also has a strong impact (.33), confirming the hypothesis that the general norms of democracy do force some constraint on to specific applications of tolerance. There is a slight tendency for conservatives to exhibit less tolerance, all other things being equal, but the coefficient is small (-.10) and is statistically insignificant. We assume that the only reason conservatives are less tolerant than liberals is that they are less supportive of the general norms of democracy, since ideology and support for general norms are strongly related (-.41).

Psychological security has the hypothesized effects on political ideology and general norms. Respondents who are less dogmatic and have greater self-esteem tend to be both more liberal and more likely to support the general norms of democracy above and beyond that expected by their liberalism. Personality's direct effect on the general norms construct is in fact quite powerful, suggesting that support for the norms of democracy is not purely cognitive, nor is it purely a function of political ideology—the affect component of personality has a powerful influence on this support. Unexpectedly, psychological security and perceptions of threat are unrelated to one another. Our expectation, that perceptions of threat would be greater when the level of inner security is low, is clearly proved wrong.

The first question mark—whether psychologi-

Table 3. Epistemic Correlations among Indicators in Political Tolerance Model

Theoretical Construct	Indicator	Epistemic Correlation
Social Status	Occupation	.53
	Income	.87
Psychological Security	Value-Actualization	.43
	Self-Esteem	.67
	Dogmatism	.81
	Trust in Others	.50
Conservatism	Self-Placement	.44
	Least-Liked Group	.58
General Norms	Minority Rights	.44
	Majority Vote	.53
	Equal Legal Rights	.32
	Free Speech	.58
	Fifth Amendment	.32
	Legal Protection	.24
Perceived Threat	Honest/Dishonest*	.67
	Trustworthy/Untrustworthy	.63
	Safe/Dangerous*	.89
	Non-Violent/Violent	.62
	Good/Bad	.66
Political Tolerance	Six-Item Tolerance Scale	.89

Source: Computed from 1978 Political Tolerance Survey, conducted by National Opinion Research Center, University of Chicago. Complete listing of the actual items, and the questionnaires, are available from the authors.

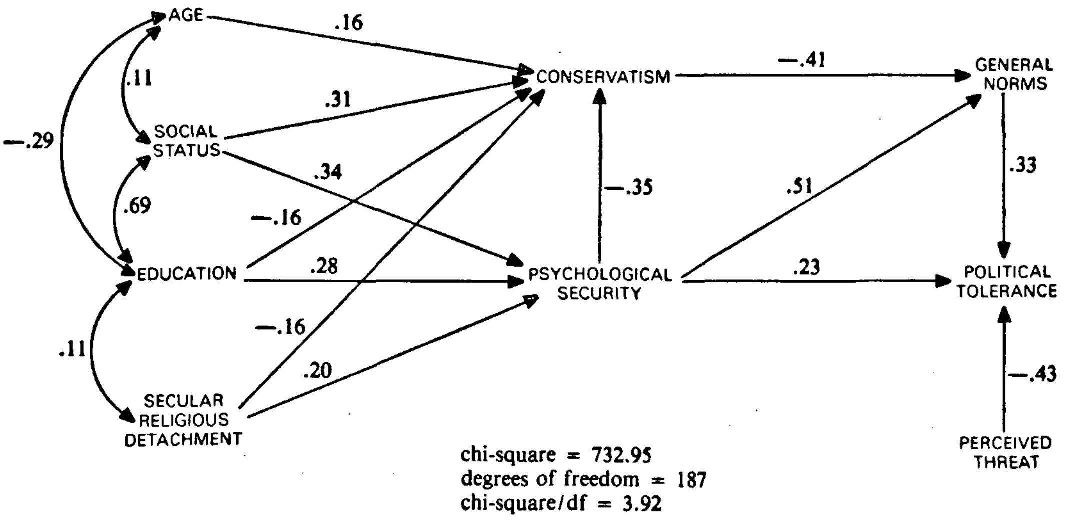
*Correlated error terms between these two items, r = −.13.

cal security has a direct influence on political tolerance—is answered in the affirmative (.23) in addition to its rather powerful indirect effect through the general norms construct. Persons with flexible, secure and trusting personalities are much more likely to be tolerant beyond that expected because of differences between them and people with opposite personalities as a consequence of the indirect effects through general norms of tolerance. The evidence is thus consistent with Sniderman's results (1975).

In Figure 2 four exogenous variables are included in the model—secular detachment, education, social status, and age. All but the third are single-indicator variables for which we assume perfect measurement. There is one significant correlation among error terms, here between income and age. Since age is assumed to be perfectly measured, the correlated error is between the error terms for the indicator (income) and the construct (age).

None of these exogenous variables has *any* direct effect on the constructs for general norms, political tolerance, or perceived threat. However, psychological security is strongly influenced by education (.28) and social status (.34); and political ideology is affected by social status (.31) while the impact of education is minimal (-.16). These results are discussed elsewhere (Sullivan et al., 1982), especially the finding that an individual's social status is positively related to the degree of conservatism, but is negatively and only indirectly related to psychological factors. Space considerations preclude the extended discussion necessary to unravel these relationships.

The model fits the data quite well, with a chi-square to degrees of freedom ratio of 3.92, generally considered acceptable.[7] The R^2 for the endogenous variables are: psychological security, .42; conservatism, .22; general norms, .60; and for political tolerance, .57. So the model does go a long way toward explaining support for the general



Source: Computed from 1978 Political Tolerance Survey, National Opinion Research Center, University of Chicago.

Note: Disturbance terms have been omitted for sake of clarity.

$x^2 = 732.95$
degrees of freedom = 187
$x^2/df = 3.92$

Figure 2. Model of the Sources of Political Tolerance

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[7] While the chi-square statistic provided by the LISREL program does yield a probability level for evaluating the goodness-of-fit of the model, this is not very useful in practice since the chi-square is a direct function of the sample size. Thus, for the number of cases we have here, any non-trivial model will fail to pass this test of statistical significance. A more useful test of fit in this case is to compute the ratio of chi-square to degrees of freedom. As a rule of thumb, ratios of less than five (for this number of cases) are indicative of a well-specified model (see Wheaton et al., 1977).

norms of democracy and specific applications of these norms to least-liked political groups.[8]

Conclusions

We have examined the impact of social, psychological and political variables on levels of political tolerance toward people's least-liked political groups, as reported by a national sample of respondents. From the analysis reported herein, we conclude the following:

1. Social variables generally have very little impact on individuals' support for the general norms of democracy or for political tolerance. Such variables have no direct impact, and have only minor indirect effects, mediated through such variables as political ideology and personality. Variables such as sex, region, race, and size of city, which previous research has suggested are strongly related to political tolerance, do not correlate with political tolerance once the content-controlled measurement strategy is adopted. Secular detachment, education, and age have a very small indirect impact on political tolerance, while, surprisingly, social status has a somewhat larger indirect impact, as it is one of the major factors influencing personality and political ideology.

2. Personality variables have both a powerful indirect impact on tolerance, through support for general norms of democracy and a fairly strong direct impact on political tolerance. Our results are quite consistent with those reported by Zellman and Sears (1971) and Sniderman (1975). We were unable to separate various aspects of personality, to examine the relative impact of self-esteem and self-actualization, for example, in order to test the relative merit of the Sniderman (1975) and Knutson (1972) theses. The presence of both indirect and direct effects *suggests* that the impact of personality is exhibited both by improving the learning of society's political norms, as suggested by Sniderman, and directly through the affective aspects of personality, as suggested by Knutson. We cannot press this line of inquiry too far, but it certainly suggests that both social learning and more traditional psychoanalytic and humanistic formulations should be taken seriously in any examination of political tolerance.

3. Two political variables—general norms and perceived threat—have a strong direct impact on political tolerance, though other political variables such as political interest, information and participation had no discernible impact. Our work is thus consistent with Lawrence's finding (1976) but somewhat inconsistent with the conclusions of Prothro and Grigg (1960) and McClosky (1964). Surprisingly, perceived threat is an exogenous variable in the model and thus is not merely or even partially a projection of psychological insecurity onto disliked political groups. The common view that inner needs dictate perception of threat is clearly not appropriate here.

On the other hand, general norms are strongly influenced both by political ideology and personality. Support for the general norms of democracy are thus partially the result of a political-ideological calculation of the importance of tolerance compared with other political values, but also partially the result of the degree to which individuals are open, flexible, and secure personally. Surprisingly, education does not have a direct effect on general norms, and even its indirect effects through ideology and personality are weak. It is plausible to suggest, therefore, that support for general norms is more the result of personality predispositions than of cognitive processes such as might result from civic education.

We by no means regard this analysis as definitive and the debate as closed. We do, however, suggest that we have taken the analysis a step further by adopting a content-controlled measurement strategy and by working with a more fully specified multivariate model, particularly one which takes an explicit measurement model into account in conducting the analysis.

The varieties of democratic theory are many and it would be inappropriate to say that these findings uniformly and consistently support any of these theories. However, some brief suggestive points may be made. The failure of political involvement—broadly investigated to include information, interest and participation—to have any direct or indirect effect on political tolerance, and the weakness of the ability of education to strengthen political tolerance suggests that participatory and populist versions of democratic theory have placed too much emphasis upon the presumed salutory impact of these variables, at least with regard to the American setting. On the other hand, the failure of measures of social status to play a powerful role in differentiating the politically tolerant from the intolerant suggests that conservative variants of democratic theory have placed too great a weight on the ability of the "better classes" to sustain the political norms of tolerance.

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[8] In our national sample, we included a subset of four items from Stouffer's 15-item tolerance index. When a scale of these four items is used in the model, several important differences occur. Briefly, the Stouffer index produces a significant negative path between conservatism and tolerance, confirming our suspicion of bias in the Stouffer items. We elsewhere describe this and other differences more fully (Sullivan et al., 1981).

Appendix

Since we had several measures of many of the important constructs in our model, we wanted to take advantage of this excess information to estimate the parameters of the model free from the effects of measurement error. Multiple-indicator models are appropriate in such a situation but the complexity of the model means that more sophisticated methods are needed (Sullivan and Feldman, 1979). To accomplish this, LISREL requires the specification of three sets of equations. The first is the set of equations giving the (hypothesized) relationships among the *true* or latent variables. To define those latent variables, two other sets of equations are necessary. One set specifies the *indicators* of the endogenous (dependent) variables as functions of these variables' true scores plus a random error component for each indicator, while the other set of equations does the same for the indicators of the exogenous (independent) variables. Thus, on the basis of these three sets of equations, all the relationships in the main and auxiliary (measurement) theories may be clearly specified as one of three types: free parameters (to be estimated), fixed parameters, and constrained parameters (unknown but set equal to some other parameter to be estimated).

In this form it is not possible to estimate directly the free or constrained parameters since there are too many unknowns in the equations. It is possible, however, to derive the variance-covariance matrix for the indicators on the basis of the hypothesized factor structure. Details may be found in Jöreskog (1973). This derived variance-covariance matrix should then equal the observed variance-covariance matrix of the indicators assuming correct specification of the model. The problem then becomes one of estimating the values of the free and constrained parameters so as to maximize the fit between the derived and observed variance-covariance matrices. If the model is identified, maximum-likelihood estimation procedures are used because for large samples these estimates are consistent (unbiased), efficient (having as small a variance as any other estimator), and approximately normally distributed. The estimators are also scale-in-variant, meaning that if the units of measurement have no real significance, as is the case here, the correlation matrix may be analyzed instead of the variance-covariance matrix. And finally, maximum-likelihood estimation generates a convenient statistical test for evaluating the adequacy of the model, the likelihood-ratio test which, for large samples (the case here) is distributed as chi-square and allows a test of the null hypothesis that the model specified by the fixed and constrained parameters is a perfect fit to the observed data. This simultaneously tests the fit of the structural relationships *and* the measurement component of the model. A large chi-square indicates that the hypothesized model does *not* fit.

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Issue Evolution, Population Replacement, and Normal Partisan Change

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Students of American political behavior have usually turned to "critical election" realignment theories to explain the dynamics of long-term change in the party system. These theories are problematic on both theoretical and empirical grounds. A theory that can account for change within the context of a stable electoral system is required. We propose a theory of issue evolution that explains normal partisan change, and illustrate the theory by examining the impact of racial issues on party identification. The theory looks to the evolution of new issues as the stimulus of partisan change and to the continuous replacement of the electorate as the mechanism through which such change is effected. In a comparison of three theoretical models of issue evolution, we conclude that racial desegregation has provoked a subtle but permanent change in the recruitment of new members of political parties.

Students of American political behavior have usually turned to "critical election" realignment theories to explain the dynamics of long-term change in the party system. Realignments, according to most theorists (Burnham, 1970; Key, 1955; Sundquist, 1973), are precipitated by the emergence of new issues about which the electorate has intense feelings that cut across rather than reinforce the existing bases of support for the political parties. It is often assumed that these cross-cutting issues, fueled by major, often highly disruptive social and economic conditions, lead to a fundamental alteration in the party system in that the majority party can no longer command decisive electoral support. One party's previously dominant position is ultimately usurped by a minority or third party, which becomes the chief beneficiary of the unfolding realignment process. According to these theories, partisan realignments involve not only massive changes in voting behavior, but more importantly, abrupt and permanent transformations in the party system as voters are moved to discard old party attachments in favor of new ones.

While realignment theories offer a plausible account of partisan change, we find them problematic both theoretically and empirically. The "critical" conception of realignment is oversimple and incomplete. Pushed to logical extremes, critical realignment models imply that all political eras are characterized by either radical change or none at all. Periods of rapid political destabilization are followed by generations when nothing happens.

Political theorists have not of course explicitly argued that "nothing happens" between realignments. Indeed, the cyclical realignment notions of Burnham (1970) and Beck (1974) explicitly postulate precursors to realignment. But given our understanding of the processes of attitude change, the discontinuity of critical realignment theories is not parsimonious. The linchpin of dichotomous notions of change is of course the event of spectacular magnitude, which temporarily moves the system from one state ("nothing happens") to another ("change"). What is problematic here is that some of the causal events are not very spectacular—what happened in 1896?—and quite a number of truly spectacular events (world wars, political assassinations, race riots in the cities) are followed by undisturbed party alignments.

Realignment theories are also deficient on empirical grounds. Most simply, there is no hard evidence supporting the postulated large-scale partisan conversions, since all realignments occurred before we began seriously to study individual party identification. Moreover, Kristi Andersen's analysis of the New Deal realignment period—the one case from which we can even extrapolate attitudinal data—sharply undercuts the notion of

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massive partisan conversion (Andersen, 1976, 1979). The Republican party may have been the majority party before the realignment and the Democrats the majority party afterwards, but it is not the case that large numbers of Republicans became Democrats. Instead, Andersen argues that the partisan shift resulted from the mobilization of previously inactive partisans.

Realignment theories have produced a serious empirical anomaly as well. If one assumes that no realignment has occurred since the formation of the New Deal party system, one is hard pressed to account for changes of massive proportions which have occurred. The Republican party, nearly equal in numbers with the Democrats after the New Deal realignment, *is now less than half of what it was.* And the numerically negligible category of citizens declaring independence of partisan affiliation is now several times what it once was. These changes are of a magnitude that quite probably dwarfs the (unobserved) changes that occurred *during* the realignment. Explaining significant change over an extended period requires a theory of *normal* partisan change. Otherwise we are left with the palpably wrong conclusion that "no change" occurred or the prospect of searching for an unnoticed realignment.

A Theory of Normal Partisan Change

Clearly, an alternative theory of partisan change is required, a theory that can account for change within the context of a stable electoral system (Shively, 1979). We propose here a theory of issue evolution that explains normal change in the party system, and illustrate the theory by examining the impact of a particular evolving issue.

Drawing on V. O. Key's often-neglected article on secular realignment, we argue that issues can bring about significant political change by producing slow, steady shifts in the partisan complexion of the electorate. Through gradual replacement of the electorate, political issues have the capacity to stimulate processes that, in Key's apt description, "operate inexorably, and almost imperceptibly, election after election, to form new-party alignments and to build new party groups" (1959, pp. 198-99).

The theory of issue evolution is a theory of normal partisan change, emphasizing that the composition of the electorate is continuously undergoing alterations that, over the course of several decades, may have profound implications for the party system. As Key wrote (1959, p. 198):

> Events and communications of political import play upon the electorate continuously; election returns merely record periodic readings of the relative magnitudes of streams of attitudes that are undergoing steady expansion or contraction. Some elections may be "critical" in that they involve far wider movements and more durable shifts than do other elections. Yet the rise and fall of parties may to some degree be the consequence of trends that perhaps persist over decades, and elections may mark only steps in a more or less continuous creation of new loyalties and decay of old.

The theory looks to the evolution of new issues as the stimulus of partisan change and to the continuous replacement of the electorate as the mechanism of change. Every passing day marks the exit of old voters from the eligible electorate and the entry of new ones. And the "streams of attitudes" characteristic of those going out are not likely to be matched by the attitudes of those coming in for the simple reason that young voters can respond to new issues relatively unconstrained by the powerful anchor of established partisan predispositions.

Normal partisan change, according to this view, results from the evolution of new issues coupled with the process of generational replacement. Both of these factors account for the gradual character of the partisan transformation. New issues by themselves are probably not sufficient. While they may lead to temporary defections in voting behavior, new issues alone are unlikely to change fundamentally the long-standing party identifications of older voters (Converse, 1969). Nor does significant secular change develop merely as a response to population replacement. The socialization of new voters into the existing partisan alignment is likely to be substantially continuous so long as the issues underlying the existing political cleavage remain predominant (Irvine and Gold, 1980). New issues thus create the opportunity for political change; new citizens are its most likely agents.

Although built upon the concept of "secular realignment," the issue evolution theory thus differs from Key's notion in postulating the "issue attitude" as the cause of change and replacement the process through which it is effected.[1] These postulates are consistent with the spirit of Key's theory, but exploit well-developed theories of party identification and longitudinal measures of attitude, neither of which was available when Key wrote. The theory is in line with Andersen's focus on new people as the mechanism of partisan realignment (1976, 1979), but differs in emphasis on a continuous rather than episodic process. The abnormal changes associated with the New Deal realignment (i.e., the rapid mobilization of the previously inactive) are, like Key's "wider movements and more durable shifts," a special case of the normal pace of replacement.

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[1] It should be noted that secular partisan change among the electorate probably occurs in several different ways, only one of which is highlighted in this article. Such change, may for example, result from demographic trends in which one social group with distinctive partisan loyalties grows more rapidly than other groups in the population.

The evolution of party differences on a political issue has little of the drama and none of the upheaval associated with critical election realignments. Nor do issues that have interesting evolutionary histories necessarily have powerful short-term effects on voting behavior. If issues develop and evolve slowly, the snapshot picture given of them in cross-sectional designs is bound to underestimate their effect, a problem that concerned the authors of *The American Voter* (Campbell et al., 1960) early in history of mass behavior research. Only a longitudinal perspective can capture the long-term consequences of evolving issues.

Properties of Evolving Issues

All political issues can be described by a general evolutionary model; they emerge, develop, and are (sometimes) resolved within the political system. Yet many policy conflicts are so non-salient that they never capture the attention of mass electorates. Even salient issues, moreover, are sometimes too short-lived to exert much influence on underlying partisan loyalties. They affect immediate voting decisions, but leave the link between citizen and party unchanged. Some issues reinforce the existing party alignment, and are a source of stability rather than change in the party system.

Issues that bring about visible rearrangements in the partisan composition of the electorate would seem to share certain qualities. Three such properties are these:

1. *Issue preferences must be deeply felt.* Preference for one policy option over another is not sufficient to recompose the electorate. Intense feeling is required. That intensity requires appeal to potent symbols to activate less-than-political masses. The issue must have penetrated public consciousness to an unusual degree or it will have too little impact to be noticeable—even in the long term.

2. *Parties and candidates must take up visibly different positions on the issue.* No matter how salient a particular policy conflict happens to be, it is unlikely to foster a partisan evolution if opposing parties and candidates cannot be differentiated on the basis of the issue. This differentiation may come about because one of the parties chooses to ignore the issue while the other party takes a strong stand, or the parties may adopt positions that differ in some degree. The important point is that partisan evolution is likely to go hand-in-hand with issue differentiation, for the latter gives citizens the opportunity to make party choices on the basis of policy considerations. Under these circumstances, parties are likely to recruit new identifiers with distinctive issue positions. The process is reciprocal and interactive; issue distinctiveness allows parties selectively to recruit new identifiers which, in turn, leads to further party differentiation on the issue. Converse (1975, p. 111) has argued that "the degree of objective party differentiation on major issues" is "very critical in the shaping of public opinion"; it is equally critical for the partisan evolution of political issues.

3. *The issue must be long on the political agenda.* Rapid partisan change is unlikely under any but the most catastrophic social and economic conditions, because a number of inertial processes work against it. One restraining condition is that the limited turnover of party elites, and therefore of party elite attitudes, restricts the ability of parties to move quickly to new issue positions. Because turnover is slow, parties are heterogeneous and party issue cues mixed. Where the stimuli are mixed and slow to evolve, mass response should be more mixed and slower still.[1] Finally, because many established party identifiers will selectively perceive party issue cues, most partisan change will occur by the recruitment of new identifiers. Reorientation by generational replacement is necessarily a slow process.

Racial desegregation is one issue—quite probably the only issue in American politics today—that has all three attributes. It has nearly a full evolutionary cycle, developing from *partisan* obscurity during the New Deal itself, to dramatic significance during the 1960s, to diminished (but still strong) salience during the 1970s. It is also an issue about which preferences are deeply felt. Like few issues in American history, race touches a sensitive nerve. It is an issue, finally, on which parties and candidates have staked out relatively unambiguous positions—most notably in the 1964 presidential election. Parties and candidates are usually able to deflect attention from highly divisive issues (Page, 1978), but this has not been the case with respect to race.

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[1] The valence issue is an interesting case of speedy party positioning, but it lacks evolutionary potential because both parties rapidly move to the same position. "Law and order," for example, ceased to be of any political interest when every candidate became equally "for" it. This points out yet another requisite for the evolving issue; it must be a genuine policy conflict. Otherwise, rational party leaders (e.g., candidates) will converge to the same popular position.

The Partisan Evolution of Racial Issues

To trace racial attitudes and party identifications over time, we have used the 1972 SRC/CPS National Election Study because it includes the richest and most varied collection of racial attitude materials. We derive two racial scales from a factor analysis of these diverse items. The first taps attitudes toward desegregation, both in general and in the traditional battlegrounds of schools, jobs, housing, and so forth. The second racial factor, more diffuse and affective, taps attitudes oriented more toward racial protest (and protesters) than the substance of racial policy.[3]

We employ the racial scales, along with personal histories of party identification, to reconstruct the racial attitudes of the electorate for each presidential contest from 1952 to 1972. We recreate the electorate for each presidential contest back to 1952 by reclassifying 1972 respondents according to their reports of party identification change and by eliminating those not old enough to vote in each reconstructed year.[4] By tracing racial attitudes by changes in party identification, this reconstruction technique allows us to determine the racial component of long-term partisan change.[5]

The recreated electorates *cannot* be taken as representative of actual electorates for any given year, but only of those young enough to be sampled in 1972. They do not tell us accurately what past electorates were, but they do, within the limits of respondent recall, tell us where the 1972 electorate came from. Apparent evidence of secular change in racial liberalism particularly should be ignored, for it may well be an artifact of the biased age distributions of the simulated electorates.

Figures 1 and 2 trace the two dimensions of racial attitudes from 1952 through 1972 for self-identified Republicans and Democrats.[6] Figure 1 suggests that the dimension we have called "Black Protest Scale" separates Republicans from Democrats and has always done so. The slight widening of the gap between partisans appears to reflect the infusion of young voters into the Democratic ranks. However, because the partisan differences are nearly constant—the lines nearly parallel—Figure 1 suggests continuity along this dimension of racial attitude.

The trends of Figure 2 are not so easily dismissed. We have already noted the danger of inferring secular trends from these data, but the observed trends do establish a baseline against which changes among subgroups can be judged. That baseline highlights the one major instance of aggregate change in racial desegregation attitudes contrary to the trend, a move to racial conservatism by Republican identifiers in 1964.[7] That movement created a gap between the parties that has never been closed. After at least a decade of similarity to the Democrats, Republican identifiers moved to the racial right in 1964 and stayed more or less in that track while other elements moved uniformly to the left.

The divergence of the parties on the traditional segregation/integration dimension is not large. Neither party can accurately be called the party of segregation or integration. But a divergence did occur, and it went against a century of associating racism with the Democratic party.

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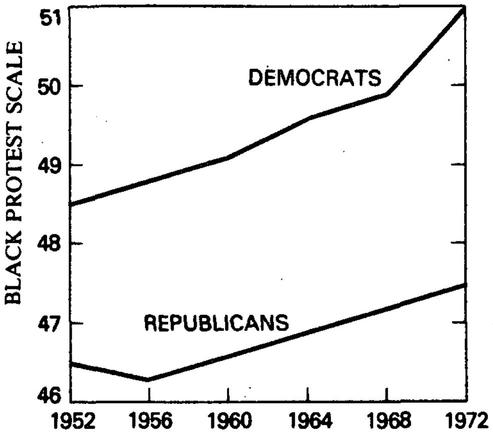
[3] See Carmines and Stimson (1980) for the factor derivation. It should also be noted that racial policy conservatism is not necessarily "racism." The primary referent of our measure is "government." Racism—not well measured in the survey tradition—is doubtless associated with racial policy conservatism, but the degree of association is an empirical question. The distinction is particularly important in assessing attitude trends. The new Republican racial conservatism may, for example, be a considerably different species from the old Dixiecrat variety.

[4] This is a variation of a technique employed by Kristi Andersen (1976, 1979). It differs in that it does not pool multiple surveys for the reconstruction and does not weight respondents by age.

[5] It should be noted that the reconstruction methodology is not without problems. Most importantly, Niemi, Katz and Newman (1980) have shown that there is substantial inaccuracy in the retrospective reports of party identification, particularly with regard to the full 7-point party identification scale. This inaccuracy, however, contributes only a minimum amount of unreliability to our analysis because: (a) we are not distinguishing between shades of party identification, and (b) the party changers are a very small group compared to the newly eligible partisans. Brody (1977) also finds that there is substantially greater stability in the "direction" of party identification than in its "strength."

[6] "Leaners" are treated as independents and, as a result, are excluded from the analysis. Thus the category self-identified Democrats includes both strong and weak Democrats, just as self-identified Republicans combines both strong and weak Republicans.

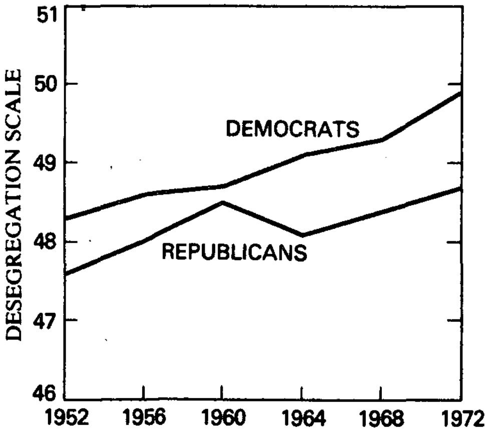
[7] We use the term "move" loosely. We cannot discriminate between individual attitude shifts and aggregate shifts caused by the gain and loss of identifiers. We assume the latter process is the major one.



Source: Computed from data collected in the 1972 Center for Political Studies Election Study, University of Michigan.

Figure 1. Attitudes toward Black Protest by Year

Because we can trace the entrance of new party identifiers over time, we can highlight the larger attitude differences among *new* partisans that are considerably diminished when aggregated with the old. In Figure 3 we plot the aggregated attitudes toward desegregation among new identifiers, by year and cumulatively since 1956. New party identifiers are in the main newly eligible voters, but the category also includes a very much smaller group, made up of those, formerly Independent, who have joined a party, or who have moved from one party to the other.



Source: Computed from data collected in the 1972 Center for Political Studies Election Study, University of Michigan.

Figure 2. Desegregation Attitudes by Year

After attracting new identifiers slightly more liberal than the Democrats in the period through 1960, the Republican party in 1964 not only moved to racial conservatism, but also did so quite dramatically. Those who report first identifying with the GOP in 1964 are strikingly more segregationist than any other group. About two-thirds of those new identifiers were newly eligible voters; their attitude distribution is, in any case, almost identical to the previously eligible voters reporting a new Republican identification. Movement away from the GOP contributed virtually nothing—contrary to much speculation at the time—to the new look of the Republican identifiers.

What is interesting about the 1968 new identifiers is not what did happen, but what did not. The appearance of George Wallace on the electoral scene in 1968 appears to have stopped for a time the racial polarization of party identification. Although the causes are no doubt multiple and difficult to sort out, it seems reasonable to assume that Wallace delayed the normal development of identification with party for his disproportionately young constituency. The Wallace candidacy removed a large number of racial conservatives form the ranks of new identifiers. The desegregation attitudes of those who did form identifications are slightly more liberal than would be expected and, more importantly, undifferentiated by party. Ideologues of the left may also have been slow to identify in 1968 because of the McCarthy/Kennedy insurgency in the Democratic party. But the number of new identifiers is too small to permit systematic analysis of these speculations.

Whatever was the case in 1968, the alignment of desegregation attitudes with party identification began again in 1972. Although the change was considerably less dramatic than in 1964, the Democratic party again went against its southern heritage to recruit distinctively more liberal identifiers and the Republican party moved slightly to the right.[8] That turn toward the right is less slight in contrast to the attitudes of other new identifiers; a small move to the right is stark against a trend in the other direction.

The net effect of adding new identifiers is shown in the cumulative portion of Figure 3. Since the Republican party has attracted both more liberal (before 1964) and more conservative

[8] The pattern is obfuscated by the fact that new Republican identifiers, while racially conservative relative to other new identifiers, are liberal relative to "old" Republicans. Thus the party in aggregate became more liberal by recruiting the most conservative group of new identifiers.

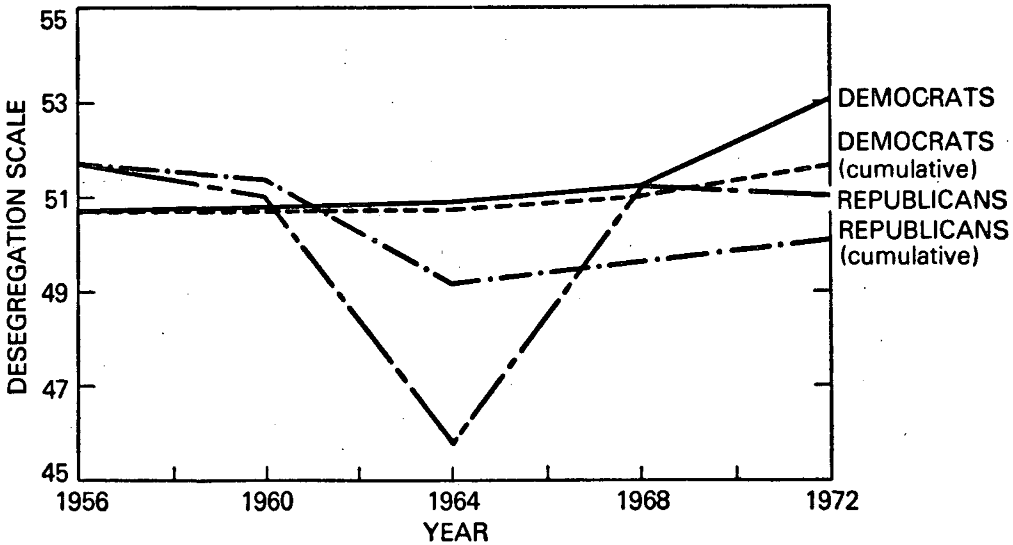
identifiers over the 20-year span, the net effect—an average of both trends—is not large. But the inter-party differences in racial attitudes in 1964 and beyond are so large that they easily outweigh the earlier pattern. And the new identifiers (of Figure 3) today comprise more than half of each party's base. Thus we have a change of some permanence, for even if race now ceases to be a salient aspect of partisan divisions, the changes already effected will be carried into the future by the inertia of established loyalty.[9]

1976 and Beyond

The 1972 electoral contest is the centerpiece of this analysis for a number of reasons. It was the first U.S. presidential election subsequent to the electoral period when racial issues were generally believed to be highly salient. Thus it is important for us to speak to the contention that 1972 marked the end of racial politics. The quantity and diversity of racial attitude materials that the 1972 CPS study offers also are unsurpassed in the national election study series. And with neither Nixon nor McGovern closely identified in the public mind with the racial politics of the sixties, the 1972 study offers an opportunity to study the long-term impact of race relatively free from short-term disturbances.

The 1976 election, on the other hand, was clouded by Jimmy Carter's identification with southern regional pride, culture, and politics side by side with appeal to and support from black voters. This confusion makes the 1976 contest a poor vehicle for the study of the partisan evolution of racial issues. The 1976 CPS measures of racial attitudes also are not the equal of the earlier study. But to assert that an issue evolution was in progress compels us to look at this newest evidence.

Because we regard the 1976 contest with some suspicion, we treat it as an independent test of the issue evolution thesis.[10] Our method is as before:



Source: Computed from data collected in the 1972 Center for Political Studies Election Study, University of Michigan.

Figure 3. Desegregation Attitudes of New Party Identifiers by Year and Cumulatively Since 1956

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[9] The new partisans of Figure 3 are both black and white. But the interpretation of the figure would be considerably different if it were the case that blacks accounted for much of the observed change. They do not. Inter-party differences for whites only (Democratic mean minus Republican mean) are: 1952, -.04; 1956, -2.88; 1960, -3.89; 1964, +2.32; 1968, +.38; 1972, +.63. This is exactly the pattern of Figure 3, Republican racial liberalism through 1960 and conservatism in 1964 and after. Blacks are left in the figure out of theoretical preference, but their tiny numbers rule out significant impacts on the aggregated party groups.

[10] It is a genuinely independent test in the important sense that our analysis to this point was completed before we examined the 1976 study. Our predictions are

Table 1. Attitudes on Desegregation of 1976 and (Reconstructed) 1972 Party Identifiers*

	All Identifiers		"New" Identifiers
Party	1972	1976	1976
Democrat	50.73	51.18	54.40
Republican	48.05	48.22	50.20
Difference	2.68	2.96	4.20
	($p < .001$)	($p < .001$)	($p < .05$)
N	1035	1136	114

Source: University of Michigan Center for Political Studies 1976 American National Election Study.

*Entries are mean scores on desegregation scale. Higher scores indicate stronger advocacy of desegregation measures (see n. 11).

we project a scale of contemporary (i.e., 1976) attitudes toward desegregation[11] backward in time (to 1972) by reclassifying respondents who reported shifts in party identification or who were not old enough to vote at the earlier time. There are two possible outcomes of this analysis:

1. Party differences in attitudes toward desegregation could decline or even disappear over the 1972-1976 period. That would undermine the issue evolution thesis, leading to the inference that the observed party change was either weak or transitory.
2. Party differences on this issue could increase or stay about the same over the period. That would be evidence of the continuing partisan evolution of attitudes toward desegregation.

The outcome reported in Table 1 is clear. The desegregation attitudes of Republican and Democratic identifiers are significantly different in both 1972 and 1976, and the margin of difference grows. The partisan evolution of racial issues was continuing in 1976.

Comparison of the 1972 and 1976 cross sections is muddied by the fact that they are partially overlapping samples. We can get a somewhat cleaner view of change by looking only at new partisans,[12] those who either are newly eligible to vote or who report a change in party identification subsequent to 1972. That analysis, also reported in Table 1, shows that the Democratic party continued to recruit new identifiers more favorably disposed toward racial desegregation than their Republican counterparts, a difference that is statistically significant. It is notable that this occurred in the face of Jimmy Carter's evident appeal to white southerners of traditional inclination.

Table 2 is a verbal summary of the complicated changes we have observed since 1960. It displays a

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thus a priori in more than the normal connotation of the term.

[11] The 1976 scale is composed of four items (school integration, neighborhood integration, "civil rights too fast," and [de]segregation) which were central to the 1972 desegregation scale (Carmines and Stimson, 1980) and continued in the later study. The scale is a sum of the four equally weighted items, expressed as a standard score with a mean and standard deviation of 50 and 10, respectively.

[12] Inter-party differences could also increase from the disappearance from the electorate of pre-realignment age cohorts, in effect, without continuing realignment. The separate examination of "new" partisans allows us to rule out this possible artifact.

Table 2. Sources and Consequences of Changes in Racial Attitudes, 1960-1976

	Sources of Change:		Net Difference Between Parties
	New Democrats	New Republicans	
1960	No Change	No Change	None
1964	No Change	More Conservative	New Difference
1968	No Change	No Change	Sustained Difference
1972	More Liberal	No Change	Increased Difference
1976	More Liberal	No Change	Increased Difference

Source: Authors' summary.

growing split between the parties that is by no means symmetrical, a result of one powerful move to the right by the new Republicans in 1964 and two later counter-moves to the left by the new Democrats (in 1972 and 1976)—all superimposed on an apparent secular trend for the electorate.[13] Over this 12-year period the parties have become clearly differentiable, mainly as a result of the behavior of new partisans.

Desegregation has provoked a slow but significant shift in the partisan composition of the electorate. It has realigned the coalitional basis of the party system. It is a strange sort of realignment, however, for it appears to work to the detriment of the minority party. Not a new issue cleavage to balance the scale of party strength, desegregation appears rather to cause further imbalance. Why then would a minority party deliberately raise such an issue?

Segregationists as a pool of "available" party supporters are something of a mirage. They exist, and in no small number. But the slight association of segregationist views with age (r = .15) makes a conservative appeal on this issue a dubious long-run proposition for two reasons. One is that older voters with their established party loyalties are less available for conversion than their younger counterparts. The South in the last several elections illustrates the point: Republican success in attracting votes has been accompanied by failure to secure large numbers of conversions to the party (see Beck, 1977; Campbell, 1977a, 1977b; also see Wolfinger and Arseneau, 1978).

The regularity of human mortality is the second reason why an appeal to older voters with segregationist views is dubious in the long run. Older voters, once converted, cannot be counted upon to stay in the electorate very long. To court the old and forego the young has predictably devastating consequences. And that appears to be precisely what the Republican party has done. In the period since 1960, 51 percent of those newly identifying themselves with the Republicans are *not* newly eligible voters, while the comparable figure for the Democrats is 22.5 percent. The segregationists are a tempting target; elections can be won with them. But to court them may squander a party's future.

Modeling Issue Evolution

The concept of issue evolution is dynamic; it implies an ongoing process (Przeworski, 1975). We conceive the process of the partisan evolution of an issue as:

1. A party system that is in equilibrium with respect to issue *X* at time *t* - 1, followed by
2. A disturbance at time *t* which results in the selective recruitment of party identifiers with regard to *X*, followed by
3. Either continuing selective recruitment or decay of th time *t* impact at *t* + 1 and beyond.

Each party is assumed to consist of a stable base of identifiers plus an increment[14] added at each election. Evolving issues are assumed to affect only the increment, except under extraordinary conditions. The net impact of issues on the party system is thus limited by the size of the increments affected. As a proportion of the party, each increment is largest the year it is added, declining thereafter as newer increments are added.

The constraints on issue evolution are thus severe. Few issues are powerful enough to affect the recruitment of party identifiers. For issues powerful enough at one time—the controversy over the Vietnam War may be a good example—initial effects are small, since the increment of new identifiers is only a minor proportion of the party. And even that small effect decays as the affected increment becomes an ever-smaller proportion of the party. To exert substantial change, an issue must influence the recruitment of party identifiers over an extended period.

These simple assumptions allow us to model some possible alternative conceptions of the impact of desegregation on the American party structure. With regard to the apparent partisan evolution beginning in 1964, we can examine the predictions of three models. They are, in order of impact:

Model 1: "Flash in the Pan"

> The apparent 1964 partisan change was purely transitory, perhaps only a temporary reaction to Goldwater.
>
> *Prediction:* Party differences on the desegregation issue should revert to their pre-1964 level. That is not the case.

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[13] Throughout this entire period (1952 to 1976) Independents have been more liberal in their racial attitudes than the identifiers of either party. Converse (1976) has speculated that racial conflict (as well as public reactions to the war in Vietnam) contributed to the increase in numbers of Independents beginning in the mid-1960s. Some of our analysis is suggestive of the truth of Converse's conclusion, but a thorough analysis of this phenomenon requires a different analytic strategy than that developed in this article.

[14] We use the term "increment" in preference to the familiar "cohort" to maintain the theoretical distinction between new partisans (of any age) and newly eligible partisans.

Model 2: "Incremental Shock"

The 1964 impact was permanent, but it affected only party identifiers recruited in that year.

Prediction: Since we know (a) the attitudes of the two parties in 1964, and (b) the size of the 1964 party groups as a proportion of all partisans in later years, we can predict expected inter-party differences purely as a function of (a) and (b) and the assumption that later increments are undifferentiated by racial attitudes. The aggregate difference between the parties will be at maximum in 1964, reflecting the impact of the new partisans with their distinctive racial attitudes. The difference should decline steadily (and predictably) after 1964 as undifferentiated increments are added in succession to the previous party base. The predicted decay of party distinctiveness over time appears in the center column of Table 3.

Model 3: "Issue Evolution"

The 1964 realignment was permanent, and the process it began is continuing.

Prediction: Party differences do not decay over time.[15] This will be the case if and only if differences in successive increments are greater than or equal to existing aggregate party differences. The last column of Table 3 shows actual attitude differences between the party groups at time *t* (1964) and beyond.

The pattern of decay predicted by the "Incremental Shock" model does not occur. Differences remained about constant in the confusing 1968 three-way contest. In two subsequent elections they have increased.[16] We are left with the inference of Model 3, that an issue evolution has occurred. With the 1964-1976 increments now more than a third of all party identifiers, inter-party differences are growing by the year. It is likely, given the self-generating dynamic of issue evolution, that this process will continue. Even if it does not, what has already occurred by 1976—barring major realignment along some other dimension—will be carried into the twenty-first century.

Another Look at Issue Evolution: A Cross-Method Validation

Reconstructed party identifications have dominated the analysis to this point. The reconstruction technique gives us a portrait of a mass electorate undergoing slow, steady transformation. Such transformation would be expected to yield modest (but significant) empirical evidence, and that is what we have seen. But modest evidence—

Table 3. Party Differences in Attitudes Toward Desegregation: Expected[a] and Actual

		Ratio 1964 Identifiers to All Party Identifiers	Expected Party Differences	Actual Party Differences
T	1964	1.000	.993	.993
T+1	1968	.912	.906	.968
T+2	1972	.795	.789	1.197
T+3	1976	.715	.710	2.960[b]

Source: University of Michigan Center for Political Studies American National Election Studies for 1972 and 1976.

[a] Expected differences are simply the 1964 difference (.993) multiplied by the ratio of 1964 (and earlier) identifiers to all party identifiers in each year.

[b] Estimation of the exact 1976 difference is made problematic because measurement of desegregation attitudes is not perfectly comparable to the earlier scale. The 1976 scale is composed of the same dominant items and is standardized to the same mean and standard deviation (50, 10) as the earlier scale. An alternate estimate can be derived from combining the 1972-1976 change in Table 1 (based wholly on the 1976 scale) with the actual 1972 difference. That lower estimate (1.480) also supports our interpretation.

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[15] Readers familiar with the time-series tradition will recognize Model 2 (Incremental Shock) as a (stationary) first-order auto-regressive process and the expected inter-party differences in Table 3 as a variation on the characteristic exponential decay process. Model 3 is also AR(1), but it is nonstationary (i.e., not tending toward equilibrium), which accords well with our conceptualization of issue evolution.

[16] There is some irony in the fact that distinctiveness appears to grow in the face of declining salience. It seems reasonable to speculate that the open conflict of the 1960s was prerequisite to: (1) clarified party images and hence (2) continuing selective recruitment in the 1970s.

however appropriate to expectations—may have many explanations, one of which is artifact. It is thus reasonable to ask whether these findings would be sustained outside the context of the method in which they were developed.

The obvious alternative to reconstructing is to use contemporaneous reports from voters regarding their attitudes toward desegregation and party identification. The difficulties involved in such over-time comparisons from cross-sectional data are well known (see Achen, 1975: Bishop et al., 1978; Sullivan et al., 1978). The important point for our purpose, however, is that they are a different set of problems from those associated with the reconstruction technique. We have, therefore, conducted cross-sectional analyses of each of the SRC/CPS presidential election studies from 1956 through 1976. A racial desegregation scale[17] is developed from the best items of each. The result is arrayed in Table 4 in party bases, new increments for each year, and the difference (Democratic minus Republican) between new party increments.

The patterns of Table 4 are notably convergent with Figures 2 and 3. Although only the data from 1972 and 1976 are common to both analyses, with but very small deviations the same year-to-year patterns are found in both. The Republican ("base") party emerges are more racially liberal than the Democrats through 1960, followed by a turn to the right in 1964 and beyond. The interparty differences are highly significant for 1964, 1968, and 1976. But more important, the pattern across all six elections is as predicted.

The changes are again more visible among the new partisans, a category that includes the newly eligible and those reporting a recent party change. As is also shown in Table 4, those newly identifying with the Republican party tended to be more liberal in 1956 and 1960, but an abrupt

Table 4. Desegregation Attitudes of Partisans, Old and New

	Democrats		Republicans		Democratic Minus Republican Difference in Increments
Year	Previous Base	New Increment[a]	Previous Base	New Increment	
1956	49.132	49.606	50.252	51.131	−1.525
(N)	(404)	(35)	(284)	(23)	
1960	49.162	49.465	49.784	55.025	−5.560
(N)[b]	(640)	(59)	(436)	(27)	
1964	50.555	50.200	48.257	45.643	+4.557
(N)	(396)	(57)	(192)	(20)	
1968	51.528	53.610	48.126	50.923	+2.687
(N)	(462)	(38)	(236)	(18)	
1972	49.282	53.301	48.382	51.041	+2.260
(N)	(726)	(143)	(466)	(74)	
1976	50.808	54.403	48.025	50.202	+4.201
(N)	(645)	(76)	(378)	(38)	

Source: University of Michigan Survey Research Center American National Election Studies for 1956, 1960, 1964, 1968, 1972 and 1976.

[a] Newly eligible to vote or reported party identification change subsequent to the previous presidential election.

[b] Weighted.

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[17] Assuming measurement comparabiity across time is chancy at best; neither the items, their wording, or indeed the issue itself is constant from year to year. Thus we report the cross-sectional analysis with some trepidation and only in a secondary confirmatory role. Each desegregation scale is as before set to a standard metric (50,10). The 1972 cross section is based upon the desegregation factor we have already employed. Items used for other years are "Civil Rights Movement Too Fast/Too Slow" (1964, 1968, 1976); "Favor Desegregation" (1964, 1968, 1976); "School Integration" (1956, 1960, 1964, 1968, 1976); "Fair Treatment in Jobs/Housing" (1956, 1960, 1964, 1968); public accommodations (1964, 1968); and open housing (1964, 1968, 1976). The 1960 scale is subject to unusually large fluctuations from the combination of both few items and very few (unweighted) respondents.

change occurred in 1965. Beginning in that year and continuing unabated through 1976, newly identified Republicans were considerably more conservative on racial issues than new Democrats.[18] Differences between these much smaller groups are significant for 1960, 1964, and 1976, and nearly so for 1968 and 1972. The over-time pattern with the partisan increments, as with the base, is perfect.

The cross-sectional analysis thus tells essentially the same story as the earlier reconstructions. Partisanship and racial attitudes among the electorate are undergoing secular change; an issue evolution is in progress.

Conclusion

The picture presented of the electorate and the party system is often a quite static, if not stagnant one. Thought to be fixed in time, anchored firmly to the social structure, and dependent for vitality upon the economic system, mass political behavior is conceived of as both stationary and nonautonomous. Periodically, it is assumed, the mold is broken as abrupt and massive changes are manifested in a critical election realignment, resulting in the overturning of the old party system and the forging of a new one more responsive to the altered economic and social conditions.

Political reality, however, is considerably more complex and less dramatic than the contours painted in this oversimplified picture. The electorate is continuously undergoing small, incremental but persistent changes that over an extended period can leave an indelible imprint on the political landscape. The theory of issue evolution is a theory of normal partisan change—the gradual transformation of the party system caused by evolving issues and effected through population replacement.

How large is that change? Students of critical election realignments look for convulsive shifts in partisan alignment. What we have demonstrated is that one particular issue, racial desegregation, has provoked measurable change in the recruitment of party identifiers over a 12-year period, less than a full generation. Evolutionary change is difficult to appreciate, becuase it is never so large from one election to the next to attract much attention.[19] But its cumulative impact is substantial when extrapolated to the millions of voting-age citizens and its implications for the development of the party system are unmistakable.

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[18] Again, to be sure that these cross-sectional trends were not due solely to the behavior of black partisans, we conducted an identical analysis among new white partisans only. The *differences* in desegregation attitudes between new partisans were: 1956, -.162; 1960, -6.04; 1964, +4.06; 1968, +1.22; 1972, +.61; 1976, +3.29. These figures support the same interpretations; new Republicans are more liberal prior to 1964 and more conservative thereafter.

[19] The principal characteristic of evolutionary change in Darwinian biology is the notion that changes of considerable magnitude occur over very long periods of time by the successive replacement of generations of species, one by another. The generations differ only in the proportions of individuals possessing minor distinctive traits, but that is enough to effect large scale change over time. That is precisely the postulated causal mechanism of issue evolution.

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Incumbency, Electoral Safety, and Turnover in the House of Representatives, 1952-76

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Two hypotheses have received a great deal of attention from students of congressional elections. First, many analysts have concluded that an increase in the electoral value attached to incumbency accounts for the rise in incumbents' victory margins observed since the mid-sixties. Second, many scholars have attributed the decline in electoral turnover to the percentage increase in victory margins. Analyzing the electoral histories of 13 House cohorts (1952-76), this study reexamines these two hypotheses within the framework of a quasi-experimental design. The results presented here, in contrast to those of previous studies, cast doubt on the increased incumbency effect. Moreover, little relationship is found between an increase in electoral safety, as reflected by increased victory margins, and a decline in electoral turnover.

Most students of congressional elections have concluded that the electoral advantage derived from incumbency has increased since the mid-1960s. Longitudinal analyses of electoral margins (Born, 1979; Cover, 1977; Cover and Mayhew, 1977; Burnham, 1975b; Mayhew, 1974; Tufte, 1973; Erikson, 1972) indicate that members of the House of Representatives have shown a greater tendency to return to Washington via safe victory margins, the increase in electoral margins hypothetically due to the incumbency factor. In addition, several scholars have viewed the increase in electoral margins among incumbent House members as the primary factor behind the declining rate of electoral turnover (Fiorina, 1977b; Cover and Mayhew, 1977; Burnham, 1975a, 1974; Mayhew, 1974; Tufte, 1975, 1974, 1973).[1] The logic behind this position operates on the assumption that representatives from marginal districts are most vulnerable to partisan shifts in the electorate. Consequently, as the number of marginal incumbent districts dwindles, and the great majority of incumbents continue to seek reelection, the probability of electoral turnover declines. Thus the linkage between victory margins and turnover implies that percentage differences translate into electoral differences. Both hypotheses, then, are linked to the decline in the number of marginal districts among incumbent races: just as the increase in the number of safe districts is attributed to an increase in the electoral value of incumbency, so the decline in electoral turnover is attributed to the increase in the number of safe districts.

Explanations of the increase in the electoral value of incumbency have varied. Several imply that House members themselves have engineered the increased advantage, either through "collusive gerrymanders" during the redistricting of the 1960s (Tufte, 1974, 1973), expanded use of office perquisites such as the frank (Mayhew, 1974) and district travel allowances (Cover and Mayhew, 1977), increased attention to casework services (Fiorina, 1977a, 1977b; Mayhew, 1974), or more skilled "position-taking" (Mayhew, 1974). In contrast, other studies contend that the increased advantage finds its source not among incumbents' activities but in the electorate. According to these studies, the erosion of partisan loyalty, complemented by a larger pool of Independents, has created an electoral environment in which the incumbency cue can challenge the partisan cue in the voting calculus (Nelson, 1978-79; Cover and Mayhew, 1977; Cover, 1977; Ferejohn, 1977; Mayhew, 1974; Burnham, 1975a, 1974; Erikson, 1972). Still other research implies that the inflated victory margins of incumbents can be attributed to ineffective opposition. Often poorly financed and handicapped by visibility problems vis-à-vis their opponents, House challengers have been unable to neutralize the advantage of incumbency (Abramowitz, 1980; Mann and Wolfinger, 1980; Mann, 1978; Jacobson, 1978).

While the explanation of the increased electoral

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[1] This article distinguishes turnover that is the result of electoral competition in the general election from the overall level of turnover that may additionally result from death, retirement, or loss in the primary. Because of the relationships considered here, turnover is examined only insofar as it relates to partisan turnover in a general election.

advantage of incumbency remains unresolved, no one disputes the validity of the increase itself and few question its ramifications. To be sure, the difficulties inherent in partitioning the determinants of congressional elections undermine dependable replicability and cross-validation. Nevertheless, the methodologies employed in previous research have inhibited the development of explicit linkages among incumbency, electoral margins, and turnover.

For example, evidence suggests that the aggregate decline in electoral turnover covaries with increases in the number of safe seats (Mayhew, 1974; Tufte, 1973). But the relationship between turnover and prior safe margins is more tenuous in analyses of individual races (Mann, 1978; Erikson, 1976). Similarly, studies that have tested for an increase in the electoral value of incumbency have observed trends in either aggregate-level distributions (Burnham, 1975a, 1975b; Mayhew, 1974; Tufte, 1973) or summary statistics, such as mean percentages (Born, 1979; Cover, 1977; Cover and Mayhew, 1977; Mayhew, 1974; Erikson, 1971, 1972). The latter include the familiar "sophomore surge" and "retirement penalty" measures. Either approach, however, necessarily obscures more subtle action that could plausibly account for the increase in the number of safe incumbent districts.

This article reexamines the hypothesized relationships between incumbency and electoral safety in individual races and, in turn, between electoral safety and turnover. The method of analysis involves a comparison of the electoral patterns in 13 House cohorts (1952-76) within the framework of a quasi-experimental design. Evidence presented here conflicts with previous findings. Specifically, the results cast doubt on both the magnitude and the uniformity of the increased incumbency effect. Moreover, there is little evidence to support the hypothesis that an increase in electoral safety results in a decline in electoral turnover.

A Quasi-Experimental Design

In order to determine the electoral effects of incumbency, it is necessary first to control for incumbency. Since the electoral margins of open-seat winners are by definition not subject to an incumbency effect, they provide a baseline against which the electoral value of incumbency can be measured.

This study examines House members who are drawn from each class of open-seat winners over the period 1952-76. Though the 13 cohorts differ in that each represents a different generation of open-seat winners, they are equivalent in that each cohort initially controls for incumbency. Admittedly, insofar as open-seat winners comprise but a small fraction of the total House membership, the choice to examine only the electoral margins of these individuals jeopardizes the external validity of this study. Nonetheless, a comparison of representatives' electoral margins at the time of their open-seat victories with their margins as incumbents represents the only method in which a test for the incumbency effect can remain internally valid. Though both types of validity are desirable, internal validity is the sine qua non (Campbell and Stanley, 1963, p. 5).

The analysis focuses on 11 of the 13 cohorts, those first elected from 1952 through 1972. In the language provided by Campbell and Stanley (1963), the quasi-experimental design employed here entails an examination of members in the 11 cohorts (1952-72) before and after the test variable or treatment is introduced. The design may be diagrammed as follows:

	Time_1	Time_2	Time_3	Time_4
Group A (1952 Cohort)	O_{A1}	XO_{A2}	XO_{A3}	XO_{A4}
Group B (1954 Cohort)	O_{B1}	XO_{B2}	XO_{B3}	XO_{B4}
.	.	.	.	.
.	.	.	.	.
.	.	.	.	.
Group K (1972 Cohort)	O_{K1}	XO_{K2}	XO_{K3}	XO_{K4}

For the members in each cohort, an *O* signifies the representative's electoral margin while an *X* signifies the incumbency factor. Thus the subsequent electoral margins of open-seat winners in each cohort are examined with incumbency held constant over three points in time. Since the design allows longitudinal analysis of the electoral effects of incumbency within each cohort, it will be referred to as the *group time-series.*

The design translates into a set of alternative patterns of reelection. These patterns are displayed graphically in Figure 1. The peak in the diagram represents the member's electoral margin at the time of the open-seat victory. Open-seat winners who were reelected as incumbents in the following three consecutive elections would then fall into one of the eight patterns shown in Figure 1. Thus the design generates expectations as to the patterns that should appear given the hypothesized relationships among incumbency, electoral safety, and electoral turnover. To facilitate analysis, these eight patterns are aggregated into three categories.

Marginal open-seat winners included in pattern 1 are termed *invariant*. These are representatives who continued to be elected by marginal percentages as incumbents. Pattern 1 implies a null relationship between incumbency and electoral safety in that incumbency exerts virtually no effect on the electoral margin.

Marginal open-seat winners who fall into patterns 3, 5, 6, and 7 are termed *variable*. These are representatives who as incumbents won by a safe margin at least once but did not maintain a safe margin in subsequent elections. Thus, among these individuals, the safe margin does not predict to the size of the future margin. Because their electoral margins are variable, in that they fluctuate between marginal and safe percentages, a null relationship between electoral safety and incumbency is implied.

Safe open-seat winners who fall into patterns 1-7 are also termed *variable*. Each of these patterns would capture safe open-seat winners who as incumbents won by a marginal percentage of the vote in at least one of their three subsequent reelections. Again, for them, the safe margin does not predict to the size of the future margin. Thus for safe open-seat winners, patterns 1-7 imply a null relationship between incumbency and electoral safety.

Marginal open-seat winners who fall into patterns 2, 4, and 8 are termed *single-peaked*, which is used here to indicate *non-decreasing change* over time (Black, 1958).[2] These patterns, in contrast to those discussed above, allow the inference of a positive relationship between incumbency and electoral safety. That is, patterns 2 and 4 imply that incumbency exerts an effect, albeit a delayed one, on the electoral margin. Pattern 8 implies that the effects of incumbency on electoral margins are immediate and prolonged.

In the same vein, safe open-seat winners who fall into pattern 8 are termed *single-peaked*. This would include those safe open-seat winners who as incumbents continued to be reelected by safe margins. This pattern, with qualification, implies a rejection of the null relationship.

According to the incumbency hypothesis, patterns of reelection should have changed during and after the mid-sixties. The expectation is that the percentage of marginal open-seat winners who exhibit single-peaked patterns of reelection has increased. An increase in the percentage of congressmen included in pattern 8 would provide the strongest support. Likewise, the percentage of safe open-seat winners who exhibit the single-peaked pattern of reelection represented by pattern 8 should have increased. Though an increase in the percentage of safe open-seat winners who sustain electoral safety does not allow the increase in electoral safety to be attributed explicitly to incumbency (since these members ran safe races before they were incumbents), it would not disconfirm the hypothesized relationship. Indeed, it is reasonable to infer that an increase in the electoral value of incumbency would account for an increase in the number of individuals who sustain their safe margins. In sum, the phenomenon that suggests a relationship between incumbency and electoral safety in this design is non-decreasing change over time in the electoral margins of individual incumbents.

This design also allows for disconfirmation of a

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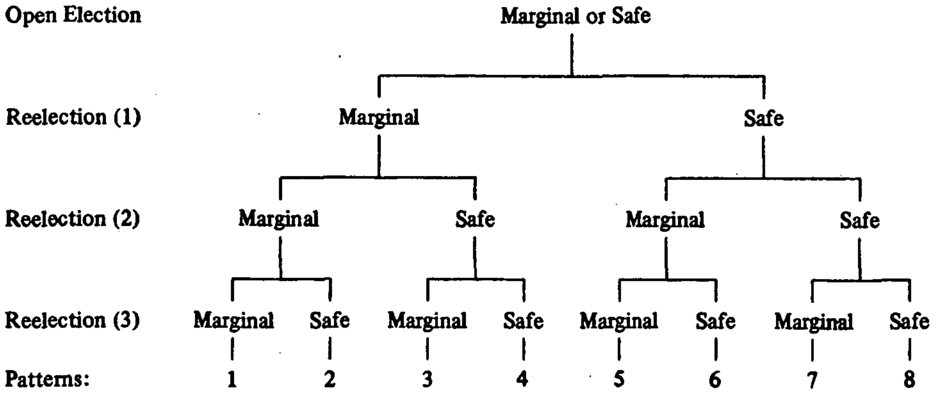
[2] Single-peakedness here merely indicates non-decreasing change; this differs from Black's use of the term.

Figure 1. Patterns of Reelection Margins for Marginal and Safe Open-Seat Winners, U.S. House of Representatives



Source: Compiled by the author.

rival hypothesis. That is, aggregate analysis implies that before the electoral value of incumbency increased, representatives from marginal open seats would largely fall within pattern 1: as incumbents, their seats remained marginal (if they were indeed reelected at all). In contrast, after the value of incumbency increased, aggregate analysis implies that representatives from marginal open seats would largely fall into pattern 8: as incumbents they acquired and sustained electoral safety. However, an increase in the variability of the reelection patterns of marginal open-seat winners (patterns 3, 5, 6, and 7) could account for the increase in the electoral safety of incumbents reflected in the aggregate. But by the same token, a trend that shows marginal open-seat winners' electoral patterns increasingly fluctuating between marginal and safe percentages probably signals an increase in the importance of idiosyncratic or district-specific factors rather than a general increase in the electoral value of incumbency.

Of course, these patterns will not include all representatives from open seats because not all of them were reelected for three consecutive terms. Here, too, the expectation is that the rate of turnover observed across cohorts should have changed during and after the mid-sixties due to the increase in safe margins among incumbents. The design would reflect the general increase in safe margins evident among incumbents in three ways: (1) an increase in the percentage of *variable* marginal open-seat winners; (2) an increase in the percentage of *single-peaked* marginal open-seat winners, and (3) a decrease in the percentage of *variable* safe open-seat winners. Were any of these trends observed across cohorts, the expectation is that the rate of electoral turnover would have declined in kind. But we may infer that a decline in turnover has resulted from an increased incumbent advantage only if both single-peaked patterns of reelection increased and cohort turnover decreased.

In sum, the following patterns would support the hypothesized relationships among incumbency, electoral safety, and turnover:

1. An increase across cohorts in the percentage of marginal and safe open-seat winners who demonstrate single-peaked patterns of reelection;
2. An inverse relationship across cohorts between electoral safety and electoral turnover, and
3. An inverse relationship across cohorts between single-peaked patterns of reelection and electoral turnover.

The general design under which this study is conducted incorporates as a special case the "sophomore surge," i.e., the increase in votes received by the first-term incumbent (see, for example, Born, 1979; Cover and Mayhew, 1977; Cover, 1977; and Erikson, 1971, 1972).[3] Since the sophomore surge measure represents a comparison of the member's electoral margin at the time of his or her open election with that obtained in the next election, it shall be referred to here as a *pre-test/post-test*.

Despite the more limited time frame, I present results for the pre-test/post-test for three reasons. First, it permits straightforward comparison to previous findings obtained under similar conditions. Second, it allows analysis of additional classes of open-seat winners, i.e., those of 1974 and 1976. Third and most important, through comparing the results obtained under a pre-test/post-test with those obtained under the group time-series, short-term effects of incumbency on electoral margins and turnover are juxtaposed with long-term effects.

Thus, under the pre-test/post-test, an increase in the percentage of marginal open-seat winners of both major parties who were reelected as first-term incumbents by safe margins would imply an increase in the electoral value of incumbency. In addition, since the marginal representative is hypothetically more vulnerable to turnover, there should have been a disparity within each cohort between the reelection rates of marginal and safe open-seat winners. If, however, incumbency emerged as a protective mechanism, this disparity should have decreased across cohorts. Just as an increase across cohorts in the marginal-to-safe pattern would attest to an increase in the electoral value attached to incumbency, so would a decline in turnover among marginal open-seat winners. Both imply that incumbency has generated increased electoral benefits. Therefore, turnover should vary inversely with electoral safety across cohorts.

In sum, the following patterns would support the hypothesized relationships among incumbency, electoral safety, and electoral turnover:

1. An increase across cohorts in the percentage of open-seat winners who demonstrate the marginal-to-safe pattern;
2. A uniform distribution of the marginal-to-safe pattern across party lines;
3. A decrease in the disparity between the reelection rates of marginal and safe open-seat winners within each cohort, and

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[3] Cover (1977), Cover and Mayhew (1977), and Erikson (1971) "adjust" the mean surge with the national swing. Born (1979) and Erikson (1972) use the swing as a measure from which to tabulate deviations.

4. An inverse relationship across cohorts between electoral safety and turnover.

The Sample

The sample includes representatives drawn from each generation of open-seat winners over the period 1952-76 who ran in contested elections. The sample excludes non-incumbent representatives who defeated incumbents because it was assumed that the incumbency status of their opponent would have a deflationary effect on victory margins. Contested open seats were defined as those in which neither candidate won more than 95 percent of the vote (Mayhew, 1974).[4] Among contested open seats, the electoral margins of cohort members were classified as marginal or safe. A margin of 60 percent or greater indicates electoral safety (Cover and Mayhew, 1977; Erikson, 1976).[5] Table 1 provides an electoral profile of open-seat winners over the period examined, and indicates that the vast majority of open-seat races were contested elections.[6] Moreover, the number of uncontested open races declined over the period. Of contested open races, a majority of House members in each year entered office within the marginal range, the two exceptions being 1954 and 1966.

Results: Incumbency and Electoral Safety

First of all, let us compare the results of this study to previous findings. Table 2 reports the

Table 1. Electoral Profile of Open-Seat Winners in the U.S. House of Representatives, 1952-76

Year	Percent Marginal	Percent Safe	Percent Contested	Percent Uncontested	Total (N)
1952	62.5 (35)	26.8 (15)	89.3 (50)	10.7 (6)	(56)
1954	36.7 (11)	36.7 (11)	73.3 (22)[a]	26.7 (8)	(30)
1956	50.0 (14)	42.9 (12)	92.9 (26)	7.1 (2)	(28)
1958	64.3 (27)	28.6 (12)	92.9 (39)	7.1 (3)	(42)
1960	65.6 (21)	25.0 (8)	90.6 (29)	9.4 (3)	(32)
1962	66.7 (30)	31.1 (14)	97.8 (44)	2.2 (1)	(45)
1964	60.5 (26)	34.9 (15)	95.3 (41)[a]	4.7 (2)	(43)
1966	43.8 (14)	53.1 (17)	96.9 (31)	3.1 (1)	(32)
1968	58.6 (17)	37.9 (11)	96.5 (28)	3.5 (1)	(29)
1970	70.3 (26)	27.0 (10)	99.9 (36)	0.02 (1)	(37)
1972	57.4 (31)	37.0 (20)	94.4 (51)	5.6 (3)	(54)
1974	68.1 (32)	31.9 (15)	100.0 (47)	0.0 (0)	(47)
1976	52.8 (28)	43.4 (23)	96.2 (51)	3.8 (2)	(53)
Mean	58.3 (24)	35.1 (14)[b]	93.6 (38)	6.9 (2.5)	(41)[b]

Source: Based on data from Congressional Quarterly's *Guide to U.S. Elections* (1952-74) and *Congressional Quarterly Weekly Report* (1976-78).

[a] Rounding error.

[b] Rounded to nearest integer.

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[4] The sample of House members reflects two further qualifications. First, it includes only those House members with no prior experience in Congress who ran in open races. This might appear self-evident given that open seats are used. But occasionally a former incumbent lost an election, and when his or her successor did not run in a later election, the former incumbent was again elected to the House. Thus, technically, a former incumbent could run in an open race. To control for all possible incumbency effects, I have excluded these individuals from the sample. Second, all candidates who ran in open special elections were excluded from the sample. Since different variables might logically come into play in special elections, for purposes of more rigorous comparison I have excluded those who entered office as the result of special elections.

[5] Though Tufte (1973), using a 53 percent cutoff point, finds a somewhat less dramatic decline in the number of marginal incumbent races, Mayhew (1974) shows that regardless of whether a 55 or 60 percent limit is chosen, the number of marginal incumbents over the 1956-72 period has halved. Though there is precedent for both the 55 and 60 percent limit, the latter was chosen here because it offers the more stringent definition of a safe margin, the effect of which is to bias the design against rejection of the null hypothesis.

[6] Analysis not presented here shows that, using the categories employed in previous research, e.g., Erikson (1971, 1972), the geographical distribution of open seats largely parallels that of the House. Data, of course, will be provided on request.

Table 2. Sophomore Surge across Marginal Open Districts in House Elections, 1952-76

Class	$\bar{X}$ Percent Surge	Standard Deviation	Percent Marginal-to-Safe*
1952	.97 (34)	4.7	20.0
1954	4.40 (10)	4.47	60.0
1956	–.21 (14)	6.99	28.6
1958	2.28 (25)	6.02	28.0
1960	4.14 (21)	4.77	38.1
1962	5.86 (29)	11.61	44.4
1964	8.71 (24)	17.50	54.2
1966	9.78 (14)	10.86	57.1
1968	8.06 (16)	7.70	50.0
1970	9.19 (25)	12.24	64.0
1972	5.73 (30)	11.84	46.7
1974	9.53 (32)	12.16	59.4
1976	12.64 (25)	15.90	56.0

Source: Based on data from Congressional Quarterly's *Guide to U.S. Elections* (1952-74) and *Congressional Quarterly Weekly Report* (1976-78).

*These percentages represent the number of marginal open-seat winners who ran safe races as first-term incumbents. The percentages were calculated against the number of marginal open-seat winners in each class who participated in the subsequent general election.

mean vote increase acquired by marginal open-seat winners when they ran for reelection as first-term incumbents. Because this measure is calculated only across marginal open seats, it differs from previous surge measures. If the increased surge found in prior research applies to the disappearance of the marginal district, this method should replicate previous findings.

At first glance, the figures on the mean surge largely substantiate earlier reports. In the early sixties the average surge gradually increased, the first notable jump occurring in 1966. Members from marginal open seats in 1964 on the average increased their electoral margin by 8.71 percent of the vote in 1966. Such a percentage increase, of course, is likely to mean the difference between a marginal and a safe victory. Subsequent cohorts (1966-76) showed a similar increase in votes between the open race and the first election as an incumbent. The magnitude of the average surge did not drop to pre-1964 levels, with the exception of the 1972 marginal open-seat winners. Even in this case, the average surge fell only below that of the 1962 cohort and remained higher than that demonstrated by all other pre-1964 marginal open-seat winners.

But more important to the linkage between electoral safety and incumbency, after 1960 the mean surge loses considerable strength as an indicator of how uniformly the average vote increase was distributed among each class of marginal open-seat winners elected after 1960 were likely deviations increase dramatically. From these figures, it may be inferred that classes of marginal open-seat winners elected after 1960 were likely to contain representatives who as first-term incumbents ran well ahead of the average surge. The outlier is, in short, a post-1960 phenomenon. Thus during the period most pertinent to the incumbency thesis, the mean vote surge obscures the general impact of the incumbency factor.

To capture the general trend across cohorts, the last column in Table 2 reports the percentage of marginal open-seat winners who acquired safe margins as first-term incumbents. The percentage of marginal open-seat winners who won by a safe margin in their first elections as incumbents gradually increased from 28 percent in the 1958 cohort to 57.1 percent in the 1966 cohort. Though the percentage dropped with the 1968 cohort, it rose again in 1970. For the post-1964 period, only in the 1972 cohort did fewer than half of the members from marginal open seats win by a safe percentage of the vote in their first reelection.

The mean percentage of individuals who exhibited the marginal-to-safe pattern across the first six cohorts (1952-62) is 36.5 percent with a standard deviation of 14.3. The mean percentage across the next seven cohorts (1964-76) is 55.3 percent with a standard deviation of 5.8. Thus, cohorts in the second period (1964-76) increasingly and consistently demonstrated the marginal-to-safe pattern.

The absolute percentages, however, are the telling indicators. The peak percentage occurred among representatives first elected in 1970, when a record 64 percent of them became safe sophomores. Moreover, there is no single breakpoint that distinguishes the mid-1960s; rather, after 1958 there was a general trend toward an increase in the marginal-to-safe pattern. Although the evidence indicates that the marginal-to-safe pattern has increased since 1964, this pattern is still far from the rule.

The partisan distribution of the marginal-to-safe pattern casts further doubt on the uniformity of an increased incumbent advantage. Table 3 reports the percentage of Democrats and Republicans from marginal open seats who acquired a safe victory margin in their sophomore election. A comparison of the mean percentages indicates that the proportion of Democrats in the second period (1964-76) who acquired electoral safety as first-term incumbents has risen only slightly. The proportion of Republicans, on the other hand, has shown a marked increase, from an average of 30.5 percent for the first period to an average of 64.6 in the second. Overall, a comparison of the means for the two parties in the two periods shows that an increase in the marginal-to-safe pattern has occurred in both parties, though more demonstrably among Republicans.

The table also shows, however, that cohorts in the second period have paralleled those in the first, insofar as in each cohort the marginal-to-safe pattern has been largely limited to members of one party. Only in the 1968 and 1976 cohorts did a comparable proportion of marginal open-seat winners in both parties run safe races as incumbents, and even then the proportion of each party that did is hardly overwhelming. "Disparity" figures are provided in the last column on the presumption that the disparity between Democratic and Republican marginal open-seat winners who went safe as incumbents should have declined after 1964 if incumbents were uniformly benefiting from an increased advantage. A comparison of the mean disparity for the two periods indicates that, contrary to expectations, the disparity between the Republicans and Democrats who demonstrated the marginal-to-safe pattern was greater in the second period.

A possible explanation of the pattern of disparities may lie in the effects of presidential coattails.[7] The expectation is that members first elected in off-years should have been systematical-

Table 3. Percentage of Safe Democratic and Republican Sophomore Representatives from Marginal Open Seats, 1952-76

	Marginal Open-Seat Winners				
Class	Democratic Percent Safe	(TOTD)[a]	Republican Percent Safe	(TOTR)[a]	Disparity[b]
1952	33.3 (5)	(15)	10.5 (2)	(19)	22.8
1954	50.0 (1)	(2)	62.5 (5)	(8)	12.5
1956	66.7 (2)	(3)	18.2 (2)	(11)	48.5
1958	27.3 (3)	(11)	28.6 (4)	(14)	1.3
1960	25.0 (2)	(8)	46.2 (6)	(13)	21.2
1962	58.8 (10)	(17)	16.7 (2)	(12)	42.1
Mean	43.5		30.5		24.7
1964	15.4 (2)	(13)	100.0 (11)	(11)	84.6
1966	25.0 (1)	(4)	70.0 (7)	(10)	45.0
1968	55.6 (5)	(9)	42.9 (3)	(7)	12.7
1970	50.0 (5)	(10)	73.3 (11)	(15)	23.3
1972	100.0 (7)	(7)	30.4 (7)	(23)	69.6
1974	50.0 (11)	(22)	80.0 (8)	(10)	30.0
1976	56.3 (9)	(16)	55.6 (5)	(9)	.3
Mean	50.3		64.6		37.9

Source: Based on data from Congressional Quarterly's *Guide to U.S. Elections* (1952-74) and *Congressional Quarterly Weekly Report* (1976-1978).

[a] TOTD and TOTR represent the total number of Democrats and Republicans, respectively, in each class of marginal open-seat winners who participated in the subsequent general election. Democratic and Republican percentages were calculated against TOTD and TOTR, respectively.

[b] Disparity is the absolute value of the difference between the Democratic and Republican percentage of safe sophomores.

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[7] I would like to thank William Panning for calling this to my attention.

ly advantaged (or disadvantaged) in the following presidential election year when they ran as first-term incumbents. So, for example, among 1966 open-seat winners, more Republicans than Democrats should have gone safe in 1968, given the 1968 pro-Republican presidential swing. Similarly, party members first elected under the auspices of an advantageous presidential swing should have suffered disproportionate losses in voting strength relative to other-party cohort members. Thus for example, Democratic open-seat winners elected in 1952 with Eisenhower should have surpassed their Republican counterparts in 1954. This reasoning explains the disparate marginal-to-safe patterning in all but two cohorts, 1960 and 1974. The expectation for both of these cohorts is that a comparable proportion of Democratic and Republican open-seat winners should have gone safe in 1962 and 1976, given the close presidential elections of 1960 and 1976. The evidence, contrary to expectations, shows a disparity between Democratic and Republican marginal-to-safe patterns. This explanation, finally, should be seen in light of the conclusions drawn in previous research that testify to the increasing divergence between presidential and congressional elections (Burnham, 1975a; Mayhew, 1974).

Thus far, the pre-test/post-test yields negligible evidence of a general increase in the electoral value of incumbency. I turn now to the more extended examination of the reelection patterns of 11 (1952-72) of the 13 cohorts obtained under the group time-series. The results presented in Table 4 show that, as expected, the percentage of marginal open-seat winners who continued to be reelected in the marginal range has shown an overall decline, with the exception of those first elected in 1966. Indeed, after 1966 this pattern of reelection died out; after 1966, all marginal open-seat winners ran at least one safe race as incumbents. This trend reflects the increase in electoral safety observed at the aggregate level in previous studies.

It can be inferred, in addition, that most House members from marginal open seats derived a cumulative electoral advantage from incumbency. In all but three classes of marginal open-seat winners (1962, 1966, and 1970), the percentage of House members with single-peaked electoral patterns was greater than the percentage with invariant or variable electoral patterns. In other words, the modal tendency in each class of marginal

Table 4. Percentage for Patterns of Reelection Margins for Open-Seat Winners in the U.S. House of Representatives, 1952-72

	Marginal			Safe	
Class	Invariant	Variable	Single-Peaked	Variable	Single-Peaked
1952	20.0 (6)	10.0 (3)	36.7 (11)	35.7 (5)	64.3 (9)
1954	12.5 (1)	25.0 (2)	50.0 (4)	22.2 (2)	66.7 (6)
1956	7.7 (1)	15.4 (2)	23.1 (3)	33.3 (3)	33.3 (3)
1958	9.1 (2)	27.3 (6)	36.4 (8)	25.0 (2)	75.0 (6)
1960	10.5 (2)	10.5 (2)	68.4 (13)	75.0 (6)	25.0 (2)
1962	7.7 (2)	34.6 (9)	34.6 (9)	53.8 (7)	30.8 (4)
1964	10.0 (2)	5.0 (1)	45.0 (9)	28.6 (4)	57.1 (8)
1966	22.2 (2)	44.4 (4)	11.1 (1)	26.7 (4)	73.3 (11)
1968	0.0 (0)	6.3 (1)	62.5 (10)	9.1 (1)	81.8 (9)
1970	0.0 (0)	47.1 (8)	29.4 (5)	16.7 (1)	83.3 (5)
1972	0.0 (0)	8.7 (2)	65.2 (15)	23.1 (3)	69.2 (9)

Source: Based on data from Congressional Quarterly's *Guide to U.S. Elections* (1952-74) and *Congressional Quarterly Weekly Report* (1976-78).

Key: All percentages for marginal and safe open-seat winners were calculated against the number of marginal and safe open-seat winners, respectively, in each class who participated in up to three succeeding general elections. The column headings represent aggregations of the following reelection patterns, where "M" and "S" signify a marginal and safe victory margin, respectively.

Percentages for marginal open-seat winners:

Invariant = MMMM;
Variable = MMSM + MSMM + MSMS + MSSM;
Single-peaked = MMMS + MMSS + MSSS.

Percentages for safe open-seat winners:

Single-peaked = SSSS;
Variable = SMMM + SMMS + SMSM + SMSS + SSMM + SSMS + SSSM.

open-seat winners (other than the aforementioned three) has been non-decreasing change in electoral margins once representatives became incumbents.

Among the 1962, 1966, and 1970 classes of marginal open-seat winners, the majority of each class was split between the single-peaked and variable patterns of reelection. Thus across the entire period when marginal open-seat winners did not demonstrate non-decreasing change in their electoral margins, they were still more likely to be reelected by a safe percentage of the vote at least once than they were to continue to be reelected by marginal percentages.

But surprisingly, we find that the percentage of marginal open-seat winners with single-peaked patterns of reelection has shown *no general increase.* This is contrary to what was expected given the hypothesized increase in the electoral value of incumbency. The highest percentages of marginal open-seat winners with single-peaked patterns of reelection may be found in the 1960 (68.4 percent), 1968 (62.5 percent), and 1972 (65.2 percent) classes. As a rule, the incidence of single-peakedness among marginal open-seat winners has remained highly erratic over the entire period.

In contrast, safe open-seat winners have shown an increasing tendency toward single-peaked patterns of reelection. The percentage of safe open-seat winners who sustained their safe margins as incumbents steadily increased after 1958, from a low of 25 percent in the 1960 class to a high of 83.3 percent in the 1970 class. Indeed, the four most recent cohorts displayed four of the five highest values of single-peakedness among safe open-seat winners.

Hence, the linkage between incumbency and electoral safety is quite different for marginal and safe open-seat winners. Safe open-seat winners increasingly sustained their safe margins as incumbents. The relationship between incumbency and electoral safety among marginal open-seat winners, however, remains problematic.

To examine the relationship between electoral safety and incumbency for marginal open-seat winners in more detail, Table 5 provides a breakdown of the three patterns of reelection termed single-peaked. Within this subset of marginal open-seat winners, after 1962 the modal tendency has been for members to acquire and sustain electoral safety as incumbents. The 1964 and 1968 classes contained the highest percentage of marginal open-seat winners who displayed this pattern of reelection, which provides the strongest evidence for the hypothesized relationship between incumbency and electoral safety. The percentages of members who continued to be reelected by marginal percentages until their third reelection steadily declined from 1952 to 1966, with the exception of 1960. After 1966 there was an increase in this pattern of reelection though the percentages remained low. In contrast, the percentages of marginal open-seat winners who ran safe races in their second and third reelections show no general trend over time; however, after 1962 only in two classes (1968 and 1972) did any marginal open-seat winners display this pattern of reelection.

The reader will note that in many cases the percentages in Table 5 reflect a very small number of representatives. Therein lies the central point of analysis in this section. The expectation is that, given the hypothesized effect that incumbency began to have on electoral margins, a great many *more* members from marginal open seats would have fallen into these patterns during and after the mid-1960s. No such trend, however, occurred. In other words, the small *N* observed for many of the cells belies the incumbency thesis.

The data presented on the reelection patterns of marginal open-seat winners in Tables 4 and 5 yield three conclusions. First, since 1964 the percentage of marginal open-seat winners who have won at least one reelection by a safe percentage of the vote has increased. Second, a greater percentage of members have immediately and, as measured here, permanently, converted their marginal open seats into safe incumbent seats. But, third and most important, *sustained electoral safety has not been the norm.* There has been no general increase in the percentage of marginal open-seat winners whose electoral margins reflected non-decreasing change over time. Moreover, fewer than half of the marginal open-seat winners over the entire period won by safe margins as incumbents and maintained those safe margins in subsequent elections, the peak percentages being in the 1964 (45 percent) and the 1968 (43.8 percent) classes. Consequently, it may be inferred that the linkage between incumbency and electoral safety is highly variable and, as a rule, tenuous indeed.

Overall, the results of the pre-test/post-test and the group time-series allow somewhat different inferences. The pre-test/post-test shows a moderate increase in the number of open-seat winners who demonstrated the marginal-to-safe pattern, which implies an increase in the electoral value attached to incumbency. In contrast, the group time-series suggests that the increase in safe margins observed for incumbents cannot be directly attributed to the incumbency factor.

Incumbency, Electoral Safety, and Turnover

The pre-test/post-test again provides a preliminary view of the relationships among incumbency, electoral safety, and turnover. Table 6 reports the percentages of each class of open-seat winners

Table 5. Percentage of Marginal Open-Seat Winners with Single-Peaked Patterns of Reelection in the U.S. House of Representatives, 1952-72

Class	MMMS		MMSS		MSSS	
1952	16.7	(5)	10.0	(3)	10.0	(3)
1954	12.5	(1)	12.5	(1)	25.0	(2)
1956	7.7	(1)	7.7	(1)	7.7	(1)
1958	0.0	(0)	18.2	(4)	18.2	(4)
1960	26.3	(5)	10.5	(2)	31.6	(6)
1962	3.8	(1)	23.1	(6)	7.7	(2)
1964	0.0	(0)	0.0	(0)	45.0	(9)
1966	0.0	(0)	0.0	(0)	11.1	(1)
1968	6.3	(1)	12.5	(2)	43.8	(7)
1970	5.9	(1)	0.0	(0)	23.5	(4)
1972	21.7	(5)	8.7	(2)	34.8	(8)

Source: Based on data from Congressional Quarterly's *Guide to U.S. Elections* (1952-74) and *Congressional Quarterly Weekly Report* (1976-78).

Key: The percentages were calculated against the number of marginal open-seat winners in each class who participated in up to three succeeding general elections. "M" and "S" signify marginal and safe victory margins, respectively.

who were reelected in the subsequent election. As was discussed earlier, the expectation is that safe open-seat winners should have displayed a higher rate of reelection than marginal open-seat winners. The disparity between the reelection rates of marginal and safe open-seat winners, however, should have begun to decline in the mid-1960s due to the increased incumbency effect.

A comparison of the reelection rates of marginal and safe open-seat winners in each cohort indicates, as was expected, that throughout the period marginal open-seat winners were less likely than safe open-seat winners to be reelected. Only in the 1954 and 1962 classes were a higher percentage of marginal open-seat winners reelected.

Paralleling the increase in electoral safety observed in the last section for first-term incumbents from marginal open seats, the rate of reelection for marginal open-seat winners tended to increase after 1964, reflected by the difference between the mean percentages for the 1952-62 period and for the 1964-76 period. The difference, however, between the mean rates of reelection among marginal open-seat winners for the two periods is minimal: 5.4 percent. This is primarily due to the inclusion of 1964 marginal open-seat winners in the calculation of the mean percentage for the second period. But, in contrast to previous studies, here we may locate the breakpoint in the 1966-68 interim. Marginal open-seat winners elected after 1964 showed the higher success rate.

If we exclude the 1964 class, the mean reelection rate for the second period climbs to 91.9 percent and the difference between the average reelection rates for the two periods in turn increases to 8.4 percent. This adjustment strengthens the predicted relationship between the increase in electoral safety observed in the last section and the decline in electoral turnover. As was expected, after 1966 the reelection record of the marginal open-seat winner increasingly approached that of his or her

Table 6. Percentage of Marginal and Safe Open-Seat Winners in the U.S. House of Representatives (1952-76) Reelected in the Subsequent Election

Class	Marginal		Safe	
1952	82.4	(28)	100.0	(14)
1954	90.9	(9)	88.9	(8)
1956	64.3	(9)	100.0	(12)
1958	84.0	(21)	100.0	(11)
1960	90.5	(19)	100.0	(8)
1962	89.7	(26)	84.6	(11)
Mean	83.5	(18.9)	95.6	(10.7)
1964	70.8	(17)	92.9	(13)
1966	100.0	(14)	100.0	(17)
1968	93.8	(15)	100.0	(11)
1970	92.0	(23)	87.5	(7)
1972	80.0	(24)	94.7	(18)
1974	93.8	(30)	100.0	(15)
1976	92.0	(23)	100.0	(22)
Mean	88.9	(20.9)	96.4	(14.7)

Source: Based on data from Congressional Quarterly's *Guide to U.S. Elections* (1952-74) and *Congressional Quarterly Weekly Report* (1976-78).

Key: The percentages for marginal and safe open-seat winners were calculated against the total number of marginal and safe open-seat winners, respectively, in each class who participated in the next general election.

safe counterpart. Thus the results of the pre-test/post-test generally support the hypothesized relationships among incumbency, electoral safety, and turnover.

The question now is: does the correspondence between electoral safety and turnover hold when we examine additional elections under the group time-series? As was reported for the group time-series in the last section, the reelection patterns of open-seat winners have mirrored the general increase in electoral safety among incumbents observed in the aggregate. Safe open-seat winners were increasingly more likely to maintain their safe margins in subsequent reelections, and marginal open-seat winners were increasingly more likely to run at least one safe race as incumbents. Thus there is ample reason to suspect that the rate of cohort turnover should have declined as hypothesized.

Table 7 reports the percentage of open-seat winners who were reelected for at least three consecutive terms. Again, there is a disparity in the reelection rates of marginal and safe open-seat winners. As expected, safe open-seat winners were consistently more likely to be reelected than were their marginal counterparts.

Contrary to expectations, however, there has been no general increase in the percentage of safe open-seat winners who were reelected. Those who entered Congress in the second period (1964-72) showed a slight edge in the probability of reelection, reflected by the difference in the mean rates for the two periods. But, as was discussed earlier, the percentage of safe open-seat winners who sustained their safe margin increased over the period 1960-70. The data in Table 7 imply that there is little relationship between that increase and electoral turnover. Among safe open-seat winners, the increase in electoral safety has not resulted in a decline in electoral turnover.

Table 7. Percentage of Marginal and Safe Open-Seat Winners in the U.S. House of Representatives (1952-72) Reelected for at Least Three Consecutive Terms

Class	Marginal		Safe	
1952	66.7	(20)	100.0	(14)
1954	87.5	(7)	88.9	(8)
1956	46.2	(6)	66.7	(6)
1958	72.7	(16)	100.0	(8)
1960	89.5	(17)	100.0	(8)
1962	76.9	(20)	84.6	(11)
Mean	73.3	(14.3)	90.0	(9.2)
1964	60.0	(12)	85.7	(12)
1966	77.8	(7)	100.0	(15)
1968	68.8	(11)	90.9	(10)
1970	76.5	(13)	100.0	(6)
1972	73.9	(17)	92.3	(12)
Mean	71.4	(12)	93.8	(11)

Source: Based on data from Congressional Quarterly's *Guide to U.S. Elections* (1952-74) and *Congressional Quarterly Weekly Report* (1976-78).

Key: The percentages for marginal and safe open-seat winners were calculated against the number of marginal and safe open-seat winners, respectively, in each class who participated in up to three of the succeeding general elections.

Nor has it among marginal open-seat winners. Despite an increase in the percentage of marginal open-seat winners who as incumbents won at least one election by a safe margin, turnover did not decline. The peak reelection rates may be located, moreover, in the 1954 and 1960 classes, both of which preceded the general increase in the number of safe margins witnessed among members from marginal open seats. Oddly enough, a comparison of the mean reelection rates across classes in the 1952-62 period and in the 1964-72 period shows that the rate of reelection during the second period has declined somewhat. Thus, on the average, marginal open-seat winners who entered the House in the second period (1964-72) were slightly less likely to be reelected than earlier classes.

Overall, and in contrast to the results obtained under the pre-test/post-test, these findings indicate that there was *no substantial difference in the reelection rates of either marginal or safe open-seat winners in the latter period.* This evidence is clearly contrary to expectations and inconsistent with the hypothesized relationship between electoral safety and electoral turnover.

Conclusions

Though the data considered here do reflect the general increase in electoral safety among incumbents, the findings are largely inconsistent with the hypothesized relationships between incumbency and electoral safety and, in turn, between electoral safety and electoral turnover. The overall results suggest, first of all, that incumbency far from assures a safe victory margin. To be sure, after 1964 more marginal open seats did go safe, but a great many did not. Furthermore, the percentage of marginal open-seat winners who sustained electoral safety as incumbents showed no general increase. Also, contrary to Born's recent conclusions (1979), these results imply that the increase in victory margins has *not* been systematically distributed across post-1964 freshman cohorts. In most cases, the marginal-to-safe pattern has been largely limited to members of one party and it is likely that the increase in margins that has been documented with previous surge data has been subject to an outlier bias. In addition, change in the relationship between incumbency and electoral safety has differed among marginal and safe open-seat winners. Safe open-seat winners now have considerably less trouble maintaining safe margins than marginal open-seat winners have in acquiring them. Thus both the magnitude and the uniformity of the increased incumbency effect may be questioned.

Second and most important, the increase in the number of safe margins observed in the reelection patterns of both marginal and safe open-seat winners has not translated into a decline in electoral turnover. Despite an increase in electoral safety, open-seat winners appear no more insulated as incumbents now than they were 20 years ago. The electoral edge hypothetically provided by increased victory margins simply has not materialized.

Parenthetically, the results on electoral safety and turnover imply that the marginal/safe dichotomy may be somewhat artificially conceived. Certainly, from the member's perspective, "last time's" election is but one of a number of factors expected to affect his or her electoral future (Fenno, 1978, pp. 10-18). And, given the reelection patterns examined here, one might question whether it is appropriate to consider the 1950s representatives with four consecutive marginal victories less "safe" than their 1970s counterparts who have acquired and maintained safe margins.

The variation in the relationship shown here between incumbency and safe margins suggests that incumbents have not been across-the-board beneficiaries of a behavioral change among voters. Neither does it imply that representatives first elected after 1964 have systematically differed from those elected before 1964. At any rate, if voters have acquiesced to the incumbency cue, or if members have worked harder at district casework, pork, or communication, it has meant little in terms of increased electability.

The electoral records reported here imply that there may be more dissimilarities among incumbents than similarities. Hence the results fit most consistently in that body of research emphasizing the importance of district or contextual factors (Jacobson, 1980, 1978; Hinckley, 1980; Mann and Wolfinger, 1980; Abramowitz, 1980; Payne, 1979; Mann, 1978). The pertinent questions become: what are the characteristics of the individuals who can sustain a high victory margin, the dynamics of their campaigns, the makeup of their districts? Since the safe open-seat winner is now the only House member who can be fairly sure of continued safe margins as an incumbent, the most powerful value of incumbency may well be that of discouraging able opposition at the outset. Seen in this light and in view of increasing retirements, the results of the open election may prove especially important in shaping the electoral profile of future district races.

Ultimately, this study warns of the traps in examining these relationships in too limited a time frame. Extended observation performed in the group time-series had especially significant implications for the inferences drawn regarding the relationship between electoral safety and electoral turnover. Using the more limited pre-test/post-test, we observed an inverse relationship between electoral safety and electoral turnover. This relationship, however, washed out, as demonstrated in the findings for the group time-series. Similarly, the relationship between incumbency and electoral safety appeared stronger in the pre-test/post-test than was borne out by the group time-series. Since election statistics easily permit extended examination of individual electoral records, it may prove useful to incorporate this approach into future designs.

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Some Social and Political Dimensions of Nuclear Power: Examples from Three Mile Island

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Public concerns about the social and political implications of energy choices have found expression in the nuclear debate. The Three Mile Island accident, in its complexity and far-reaching consequences, served to focus criticism of nuclear power that had been building up for a decade. It revealed with striking clarity that technology is a social concept to be understood in terms of social and political relationships if it is to be brought under public control. The ramifications of the accident revealed the conceptual difficulties of assessing social risks and the political difficulties in managing them. This article draws examples from the Three Mile Island accident to review several characteristics of nuclear technology, its scale and costs, its complexity, its uncertain and unpredictable physical effects, and its indirect risks. It then explores the implications for social, political and administrative institutions as they grope for ways to manage the risks of nuclear power in the context of critical public scrutiny. Finally it identifies a range of issues calling for policy research.

Energy choices have critical social and political implications. The scale, the cost, the complexity, and the nature of the risks associated with various energy sources entangle such choices in a web of political, economic and organizational problems. Decisions at every level bear on the shape of industrial and urban development, the organization of major institutions, the forms of authority, and the distribution of risks (Noble, 1977; Winner, 1980).

Public concern about the broad-reaching social and political implications of energy choices has found expression in the nuclear power debate. The persistence of opposition to nuclear power in part reflects fear about the risks of this technology and its "apocalyptic image of extinction" (Lifton, 1976, 1979), a fear that is not susceptible to measurement by comparing risks and benefits. But nuclear power also symbolizes the dilemmas of many large-scale advanced technologies, and controversy has focused on the social properties of nuclear power as well as its immediate risks.

The Three Mile Island accident of March 1979, in its extraordinary complexity and far-reaching consequences, has served to focus the criticism of nuclear power that had been building up for a decade. The ramifications of this event have shown that the concept of risk, often defined in the rather narrow framework of health and safety, must be considered in far broader terms. They reveal both the conceptual difficulties in assessing social risks and the political difficulties in managing them.

Drawing examples from the Three Mile Island accident and its aftermath, this article briefly reviews several characteristics of nuclear technology—its scale and costs, its complexity, its uncertain and unpredictable physical effects, and indirect risks. It then explores some of the implications for major social, political and administrative institutions as they grope for ways to manage the risks of this cumbersome technology in the context of increasingly critical public scrutiny. The intention is to identify a range of issues suitable for and, indeed, calling out for policy research.

Characteristics of Nuclear Power

Scale. The siting of a nuclear power plant presents special problems simply because of its scale and its staggering capital costs. In 1969, nuclear plants, projected for completion in 9 years, were expected to cost about $226 per kilowatt hour. In 1978, with an anticipated 12-year construction period, estimates increased to $1,648 per kilowatt hour.

Like any large-scale project, a power plant dominates its environment, profoundly affecting the tax structure, real estate values, and economy, as well as the physical environment of a community. Effects may be positive or negative: real estate values may rise if the installation attracts industry and new people to the area, but also they may decline if the facility is viewed as undesirable. A local community may disproportionately benefit from the construction of a power plant through a more favorable tax base or increased employment. Or it may lose out because of health hazards or environmental risks. The labor necessary to build a power plant creates new markets and the need for new services. The very scale of such a structure may thus affect the distribution of social and economic privilege (Shields, 1979).

When a project fails, such impacts become obvious. The Three Mile Island accident had an

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enormous effect on the local economy of the Harrisburg, Pennsylvania region. Local estimates of wages lost as a result of the accident ranged from $5.7 million to $8.2 million in manufacturing, and from $2.8 million to $3.8 million in the non-manufacturing sector. An estimated $6 million to $8 million were lost in revenues partly due to the decline in tourism. Losses in the agricultural sector were about $1 million. As some 34,000 people left the area, 256,000 person-days were lost. Of these, 15,000 lost pay as well (Flynn, 1979).

Orders for new nuclear plants had been declining for several years and the accident provoked a rash of further cancellations. Sixteen reactor orders in other parts of the country were withdrawn. Utilities in Ohio and Pennsylvania scrapped their plans for 4 nuclear plants despite an existing investment of $250 million.

Large-scale technological projects raise major questions of social equity. Given very long lead times, who should pay for construction costs? Are consumers today to pay for installations that will produce electricity for residents 15 years hence? The Three Mile Island accident also raised the issue of consumer liability in the case of accident. According to the President's Commission on the Accident at Three Mile Island (the Kemeny Commission), the cleanup, including emergency management and replacement of power, will cost from $1 billion to $2 billion (President's Commission, 1979). Some estimates, including the cost of building a new nuclear plant on the site, were as high as $3 billion (Technical Assessment Task Force, 1979). General Public Utilities Corporation (GPU), the holding company owning the Three Mile Island reactors, is deeply in debt and faces possible bankruptcy. Between January 1979 and January 1980, its net income dropped 51 percent and its liabilities increased 44 percent. It tried to pass costs on to consumers and won some emergency rate increases, but consumer pressure successfully limited this possibility. The increases as of March 1980 provided $150 million, and disputes continue over whether consumers or stockholders should foot the bill. Because these utilities employ 11,000 workers and control the supply of electricity in Pennsylvania and New Jersey (GPU owns Metropolitan Edison, Pennsylvania Electric Company and Jersey Central Power and Light Company), a bankruptcy could have disastrous consequences.

High capital investment encourages industrial commitment to existing technological systems, and limits the flexibility that may be necessary to adapt to safety requirements. The manufacturer of the Three Mile Island reactor, Babcock and Wilcox, had been warned of equipment problems. The pilot operator relief valve had failed at least 9 times prior to the accident. An earlier accident at a reactor in Toledo, Ohio had revealed several design problems, but the company failed to modify its technology to accommodate them. The nuclear industry is perpetually faced with accusations of falsifying records and disregarding regulations.[1] Given the high costs of construction and repair, financial penalties may be easier to absorb than the cost of major structural modifications in a nuclear facility. Indeed, the nuclear industry and its investors argue that required safety modifications have increased the cost of nuclear power to the point where new plants are financially "unattractive" to investors (Hebert, 1980).

Complexity. The complexity of a nuclear power plant, both in technological and organizational terms, and the interdependence of organizational and technical systems, contributed to the awesome difficulty of coping with the Three Mile Island accident.[2] The sophisticated organizational requirements of a nuclear power installation—the skills required to operate it, the organization of a managerial infrastructure, and the establishment of means of control—pose staggering problems of administration beyond the expertise and managerial capacity of most utilities. In this sense, the problems of Metropolitan Edison (the subsidiary of GPU licensed to manage the Three Mile Island reactor) were hardly surprising. The accident demonstrated the convoluted interrelationships between technical and human factors in the operation of technological systems and how this relationship reduces the predictability of risks and increases vulnerability. It suggested the difficulty of anticipating complications that include both operating errors and equipment failures, and of controlling breakdowns that ramify, often unpredictably, throughout a system.

During normal operations a reactor is run with the assistance of a computerized instrument and control system. Computerization is necessary to make rapid adjustments to changes in the reactor's equilibrium. The Babcock and Wilcox reactor model is especially dependent on the use of an automatic computer system because its sensitive design requires split-second reactions to small changes. The sequence of events during a minor accident at the Babcock and Wilcox Crystal River plant in Florida in February, 1980 suggests some of the dilemmas resulting from such dependence. The accident began when the computer misread a short circuit to indicate a cooling of the reactor. It responded by withdrawing the control rods and reducing the flow of coolant water. This overheated the reactor, driving the pressure up and then automatically shutting the reactor down. The pressure relief valve opened and the Emergency Core Cooling System pumped water into the reactor, eventually spilling 43,000 gallons of radioactive water on the floor. The reactor essentially "took off and did its own thing" (Marshall, 1980).

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[1] In March 1980, Commonwealth Edison Company, the country's largest utility, was indicted on charges of conspiracy and making false statements about breaches of security.

[2] The problems of managing large-scale technological enterprises have been discussed in the literature, especially in papers about innovation. See Stanfield (1976) and Horwitch (1979) for literature reviews.

At the Three Mile Island reactor, the control room had been designed so as to be useful during normal operations but not necessarily in a crisis. One hundred alarms went off in the first few minutes of the crisis. There was no way for operators to sort out the signals. When reliance on the technical systems failed, the operators were inadequately prepared to deal with the emergency. Some blame the limitations of training which focuses principally on normal operating requirements. Others wonder whether the demands on an operator during a crisis are within the range of human capability. It has been argued that accidents are, in fact, "normal," that is, intrinsic to complex, interdependent systems. To the extent that this argument holds, it warrants special concern, given the dangers that could result from a nuclear accident (LaPorte, 1980; Perrow, 1980).

Uncertainty and Unpredictability. Efforts to estimate the risks of an accident, or the severity of health hazards should one occur, are beset by technical uncertainties. The deluge of major studies on risk have only accelerated the technical debate. The Rasmussen Report (1974), commissioned at a cost of $3 million by the Atomic Energy Commission, argued that nuclear power presented only minimal risks as compared to other technologies. The report concluded that a major reactor accident was so unlikely that the overall risk was very small (1 percent per year for 100 operating reactors) even in a full-scale nuclear economy. Moreover, it claimed, most accidents would have minimal consequences for life and property. The Rasmussen report, however, faced a flood of criticism. The American Physical Society took issue with its estimation of the possible consequences of an accident. The Union of Concerned Scientists (1977) refuted its methodology and the very assumptions underlying the report. Fundamental disagreement among scientists persists, exacerbated by the potentially catastrophic nature of nuclear risk (Fischoff, 1979).

Similarly, efforts to predict the long-term effect of low-level radiation are obstructed by disagreement about the level at which radiation presents significant risks, and by inherent difficulties in sorting out its effects from other environmental insults. Here disputes among scientists have complicated the establishment of radiation standards for occupational safety.

The reaction of experts from federal and state agencies, industry, and anti-nuclear groups to the Three Mile Island accident illustrated the uncertainties about the risks of nuclear power and revealed how such uncertainty can be manipulated to serve particular ends (Nelkin, 1980). The early estimates of radiation released by the accident varied widely. On March 29, immediately following the accident, the utility detected 7 millirems of radiation, but the NRC estimated around 20 millirems, claiming that a person near the plant could have received as much as 100 millirems. In late April, the NRC reported that the radiation levels within a 5-mile radius were near normal, but on the same day a report from the airport three miles away reported radiation levels 10 times normal. A chemist from the anti-nuclear movement made an independent inquiry and found on April 11, that radiation was 5 times the normal level at a site 20 miles from the plant, and on April 16, it was 50 times normal at a site 30 miles from the plant.

Consistent measurement is important as a basis for estimating the seriousness of exposure to radiation. The variation in these reports allowed a selection of data that could support any interpretation. The very language used to describe the accident revealed the very diverse percceptions that enter such interpretations. Was it an accident or an incident? A catastrophe or a mishap? A disaster or an event? A technical failure or a simple mechanical breakdown? Utility officials talked of "miniscule" hazards, comparing the dosage to dental X-rays. Critics talked of cancer and of "the grim reality of death." Harold Denton, the chief of operations for the Nuclear Regulatory Commission (NRC), reassured the public that the radiation emission was "slight" and that sampling of milk, food, and water showed "negligible contamination." There was "no cause for alarm." At the same time, he urged pregnant women and children to evacuate the area and saw to it that potassium iodide, an antidote that blocks the accumulation of radioactive iodide in the thyroid, was on site.

Evaluation inevitably turned to a body count. In May, HEW Secretary Joseph Califano asserted that 1 to 10 additional cancer deaths (depending on controversial assumptions about the effect of low-level radiation) could be expected among the people living within 50 miles of the accident. An inter-agency task group estimated that the potential lifetime impact of the accident would be less than 2 fatal cancers and 2 non-fatal cancers among those living within 50 miles of the site.

Others estimated 20 additional cases of cancer, about half of them likely to be fatal, and one doctor predicted 50 additional deaths. Eventually, the task force studying radiation health for the Kemeny Commission concluded that the accident had a negligible health impact (President's Commission, 1979). However, locally in the Harrisburg area, where several long-term health studies are under way, debates continue over statistics. While most studies deny significant health effects, some claim increased infant morality and hyperthyroidism, and a dramatic array of health problems among domestic animals. The major finding of the Kemeny Commission, however, was the important psychological stress on the local population.

Indirect Risks. The Three Mile Island accident posed a number of unexpected social and psychological problems that must be included in an extended definition of risk. A major problem emerged following the official advice to evacuate pre-school children and pregnant women living within a 5-mile radius of the power plant. While this was only an evacuation "advisory" (that is, not an order) about 60 percent of those people living within 5 miles, 40 percent of those in the 5- to 10-mile ring around the plant, and 32 percent of those in the 10- to 15-mile ring left the area on a voluntary basis. People began to withdraw funds from the banks, overloaded the telephone lines, and pulled their children out of school (Henderson, 1980).[3]

Little thought had been given to the implications of such an action. The governor of Pennsylvania, Richard Thornburgh, expressed his concern about its possible effects:

> The movement of elderly persons, people in intensive care units, babies in incubators, the simple traffic on the highways that results from even the best of an orderly evacuation, are going to exert a toll in lives and injuries. Moreover, this type of evacuation had never been carried out before on the face of this earth, and it is an evacuation that was quite different in kind and quality than one undertaken in time of flood or hurricane or tornado. . . . When you talk about evacuating people within a 5-mile radius of the site of a nuclear reactor, you must recognize that that will have 10-mile consequences, 20-mile consequences, 100-mile consequences (Thornburgh, 1979, p. 21).

Studies of the behavioral effects of the accident found the major short-term health impact to be psychological stress and demoralization among those living near the plant and faced with continued uncertainty about their fate (Dohrenwend, 1980). Those who actually worked at Three Mile Island were the most demoralized and mistrustful people in the area, worried both about their health and about losing their jobs should the reactor close (Chisolm, 1980; Logan and Nelkin, 1980). But even in the larger community, psychiatrists reported severe demoralization and mental health problems.[4] These problems faded with time, but anger and distrust remained, exacerbated by the uncertainties over the logistics of the cleanup operation. Civic leaders have warned of violence if the utility were to vent Krypton gas from the crippled reactor—violence from a generally law-abiding and conservative population that prior to the accident had few negative feelings about nuclear power at all. The mayor of Middletown, Pennsylvania jokingly proposed that the banks add a radiation count to their displayed time and temperature clocks, but then observed, "I'm quite sure they'll never tell us the truth about the venting" (Reid, 1980). A telephone survey of area residents found that most people believed they were given misleading information by the utility and the NRC. Meanwhile, a local pro-nuclear group has formed to protest the public image that the town is in a state of hysteria (Flynn, 1980). The issue has thus polarized the communities near Three Mile Island with profound effects on social relationships.

Other unexpected risks were associated with this accident. The general difficulty of keeping track of plutonium is a problem of increasing concern in the regulation of nuclear power. Safeguards, claimed investigators from the General Accounting Office (GAO), are inadequate. Neither existing accounting systems nor physical security arrangements can prevent sabotage or the diversion of fissionable materials (GAO, 1980). These problems are exacerbated in the case of accidents. The cleanup of Three Mile Island requires that about 1500 persons, mainly contractors, have unescorted access to the site, precluding effective precautions against sabotage from within.

These social and political characteristics of nuclear power technology can serve as a framework for analyzing a variety of policy questions concerning this and other large-scale technological systems. For example, what are the social and economic impacts of nuclear power plants on their local communities? How are the costs and benefits of this technology distributed? As the industry modifies reactors, at what point does the cost of this technology become impractical? And if the risk of a penalty is more practical than compliance with costly regulations, what are the implications for safety?

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[3] According to a calculation by the Natural Resources Defense Council (1980), if an identical accident had occurred at Indian Point, New York and evoked a parallel public response in this heavily populated area, one million people would have been evacuated.

[4] This was one of the conclusions of the Kemeny Commission. The State of Pennsylvania commissioned a psychological impact study run by Dr. Evelyn Bromet of the University of Pittsburgh.

We need to know more about the relationship between technical and human factors in the operation of large-scale technological systems. How do these relationships bear on their vulnerability to accidents? If in fact accidents are normal in complex systems, is the further complexity added by new safety requirements counterproductive? The proliferation of disputes among scientists concerning the risks of nuclear power suggests another set of research questions. What is the effect of the increasing polarization of scientists on the decision-making process?

Considerable data have been collected on public attitudes towards nuclear power and on the changes in attitudes as a consequence of accidents. Relatively little work has focused on the occupational and social class correlates of such attitudes. What is the effect of confusion and uncertainty on public views toward nuclear power? What in fact is the source of persistent and often profound skepticism about this technology? Is it simply the fear of risk, or as some would claim, the apocalyptic image that links this technology to nuclear war (Lifton, 1979)? How does opposition link with political ideologies? (Nelkin and Pollak, 1980). Finally, the characteristics of nuclear power have had profound implications for a number of social, political and administrative institutions. Here too the Three Mile Island accident has suggested an array of vital issues amenable to policy analysis.

Institutional Implications

The Three Mile Island accident occurred at a time of intense popular opposition to nuclear power. It substantially contributed to the growing problems of credibility faced by the nuclear establishment—that social network comprised of industry, public officials, administrators and scientists concerned with the implementation of nuclear policy. In the country at large, the accident hardened existing attitudes towards nuclear power, and caused some increase in opposition, as well as considerable distrust in the responsible authorities. These trends continued even a year after the event. A series of polls indicated that the percentage of people perceiving nuclear power as "very safe" was 26 percent in October 1978, 21 percent in April 1979 (right after the accident) and 15 percent in February 1980. The percentage of people judging the technology "not so safe" or "dangerous" increased from 28 to 30 to 38 percent in the same set of polls. By February 1980, polls indicated that 20 percent would like the nuclear program to stop, 47 percent would allow operating plants to continue but wanted no further growth and only 22 percent wanted further expansion (Mitchell, 1980). In this context of growing mistrust about nuclear power, the accident affected the role and responsibility of a number of institutions. The nuclear industry, public officials, the regulatory system, the courts, the scientific community and the media were all involved.

The Nuclear Industry. For the nuclear industry, the future of a $100 billion investment at stake, the Three Mile Island accident presented a serious problem of public image. Thus the industry proceeded with a massive effort to assure the public that nuclear power is a safe and controllable technology. Industry representatives talked of their "energy awareness programs" and their "truth squads" (Starr, 1980). The Electric Power Research Institute (EPRI), a consortium of utilities, set up a Nuclear Safety Analysis Center to study the Three Mile Island acident and to persuade the public that nuclear power is safe and necessary. Its estimated annual budget is $8 million. The industry also set up the Institute for Nuclear Power Operations to improve reactor operating standards and to monitor utilities to assure compliance to these standards. With 200 professionals, its budget is about $11 million.

The accident stimulated a number of proposals to restructure the industry. These ranged from a single national nuclear utility, to a set of nuclear corporations—all designed to reduce the fragmentation that exists at present. Alvin Weinberg has proposed a "moratorium" on new sites through a program that would cluster nuclear plants in designated areas and restrict their construction elsewhere (Weinberg, 1980). Proposals on ways to reduce dissent have flourished: a blackout on news within a 50-mile radius of accidents, restrictions on citizen intervention in licensing hearings, preemption of local control over transport and siting, and exemption of certain Nuclear Regulatory Commission decisions from the requirements of the Freedom of Information Act.

Public Officials. The Three Mile Island accident also presented a problem of credibility for public officials. Convinced of the need to expand the nuclear program and to speed up the licensing procedures, President Carter was directly concerned with easing public skepticism about nuclear policy by demonstrating that the executive was "doing something" about risk. The problem for Carter was less to evaluate the future of nuclear policy than to assure its public acceptability. He appointed the Kemeny Commission to this end.

To help establish its own credibility, the members of this commission were selected to represent very diverse interests. Its chairman was John Kemeny, president of Dartmouth and a mathematician. Its members included a biochemist, a nuclear engineer, a physicist, a governor, an ex-presidential aide, an industrialist, a trade unionist, a sociologist, a professor of journalism, an environmentalist, and a housewife active in civic affairs. Kemeny explained this diversity in terms of the need for credibility:

> I think it would be wrong for the Commission to consist of a bunch of engineers. We are sort of a national jury on this issue and would you want the jury to consist of experts? I think it would be a mistake; you want the average American to believe what this Commission says (Kemeny, 1979).

The Kemeny Commission investigation was only one of six major inquiries that were initiated as public officials at various governmental levels faced the implications of the accident. The NRC, the Senate Subcommittee on Nuclear Regulation, and the Energy and Environmental Subcommittees of the House Interior Committee each formed special inquiry groups. In addition, the governor of Pennsylvania formed a special commission and the utilities organized a Nuclear Safety Analysis Center. Beyond these major investigations, more specialized studies were sponsored by the Department of Health, Education and Welfare, the Environmental Protection Agency, the Center for Disease Control, the Food and Drug Administration, the National Cancer Institute, and a variety of local and state government agencies. The confusion among government agencies with overlapping spheres of influence was apparent as all these groups sought to deal with the emergency and later to establish blame. The organization of the regulatory system was clearly a major problem.

The Regulatory System. The scale and complexity of a technological facility is matched by the size and complexity of the regulatory apparatus required to control it. Technologies of scale can blur the distinction between public and private interests. Given the size of the nuclear enterprise, government has become a partner in the nuclear field, developing through its contracts and subsidies a stake in the promotion of nuclear power. This partnership reduces the ability of government to exercise independent regulation and control. Regulatory agencies seeking to function in such a context are often attacked for their promotional philosophy, and the agencies responsible for regulating nuclear power are no exception. The 1974 amendments to the Atomic Energy Act reorganized the administrative structure responsible for regulation, with the explicit purpose of separating the functions of promotion and control. This legislation abolished the Atomic Energy Commission (AEC) and set up a separate Nuclear Regulatory Commission. However, much of the existing staff, with their industrial alliances, remained. A 1976 study of the NRC staff found that 65 percent of its 429 senior members had been employed in private energy enterprises holding NRC licenses, permits or contracts, and 73 percent of NRC consultants maintained simultaneous relationships with the agency and industries with NRC licenses or contracts (Kneier, 1976). The NRC, claimed this study, is tied to the interests of the industry to the extent that it cannot effectively guide a cautious and safe development of nuclear technology.

According to a study by the General Accounting Office (1980) as well as an analysis commissioned by the NRC itself (Rogovin, 1980), the agency has changed more in its title than in its essence. It has retained the AEC mentality, just as it has retained it staff and most of its procedures. Like the AEC, the NRC has been mainly concerned with expediting the development of nuclear power. It did not "burden" the industry with requirements to "backfit" older plants and bring them up to current safety standards. It downgraded penalties and enforcement sanctions. It failed to respond to questions of safety raised by some individuals within the agency. Nor did it engage in comprehensive evaluations of past operating experiences or systematically review the series of minor accidents that have plagued the development of nuclear technology. Focusing on licensing rather than generic problems, the NRC failed to establish adequate mechanisms to monitor safety-related problems, relying rather on licensees to detect and correct deficiencies without independent commission verification. As the Kemeny Commission reviewed the history of the NRC, it found it "erring on the side of industry's convenience rather than carrying out its primary mission of assuring safety" (President's Commission, 1979).

The complexity of nuclear technology and its broad-ranging impacts on the social environment create further problems of regulation. Organized to deal with specific industrial activities, regulatory agencies are unsuited to cope with systemic problems. The difficulty of responding to the Three Mile Island accident suggests the inherent managerial problems in coordinating intricate inter-organizational relationships.[5] For example, a veritable alphabet soup of public agencies was officially coordinated through an Interagency Radiological Assistance Plan designed specifically to mobilize expertise in cases of emergency. The response to Three Mile Island revealed that the existence of this plan was not well known, links between agencies were poorly established, and lines of responsibility weakly defined. At the state level, despite the existence of several environmental and health agencies, neither plans for emergency evacuation and medical care nor a capacity to monitor radiation levels were evident.[6] NRC Commissioner Joseph Hendrie described the dilemma: "We are like blind men staggering around" (Nuclear Regulatory Commission, 1979).

As a result of the accident and subsequent criticism of the NRC, its staff grew by 14 percent from 2,841 to 3,240 in one year, and its annual budget increased from $325.8 million to $423 million. Requirements were developed for additional training of reactor operators. Emergency plans include telephone hotlines to a commission emergency response center. The inspection system was improved and the structure of the commission itself reevaluated. But "improved" regulation will inevitably increase operating costs and it may also exacerbate the problems of complexity, compounding the difficulties of management and the risk of systemic effects.

The Courts. The courts have been deeply involved in the nuclear debate. The role of the courts in technological questions has generally expanded with the extension of the legal doctrine of standing. Courts are defining responsibility for damages caused by technology, imposing injunctions against possibly harmful developments, and galvanizing administrative agencies into more effective regulatory action. But legal action in many technological areas, including nuclear power, poses special problems for the courts as they increasingly become the channel through which aggrieved citizens react to unpopular technological decisions.[7]

The Three Mile Island accident generated an unprecedented legal battle to establish liability. The Price-Anderson Act provides guaranteed government subsidization of insurance liability against damage claims caused by nuclear accidents. The act set a ceiling of $560 million liability in claims. Private insurance offers protection of up to $125 million; the government is responsible for the difference. Following the Three Mile Island accident, 30 neighbors of the plant immediately filed a class action suit for a total of $560 million. Another suit against the designer of Three Mile Island sought damages of $1.06 billion. Complaints have included loss of income, devalued real estate, endangered health and "diminution of the enjoyment of life." It is argued that the limit set by Congress in the Price-Anderson Act is unconstitutional if the defendants can be proven guilty of gross negligence. Other legal battles over who should absorb the costs of accidents have been instigated by consumer advocates. And General Public Utilities Corporation, owner of the Three Mile Island plant, is suing Babcock and Wilcox to recover $500 million in damages that occurred as a result of the accident and plans to seek further damages for continued loss, estimated at $400 million. Babcock and Wilcox denies liability. The variety of issues under consideration by the courts have been vividly described by a nuclear law expert as "a can of worms."

The Scientific Community. The scientific enterprise is another institution profoundly affected by the controversial dimensions of nuclear technology. Scientific standards have long had a kind of universal appeal, serving as a model of rationality and a widely accepted basis of consensus on many policy questions. But the increasing involvement of scientists in controversial areas of public policy also has a deleterious effect on the image of technical expertise as an objective, apolitical and compelling source of information. Disputes among experts have called attention to the technical uncertainties that compound questions of risk. They have exposed the wide range of social and political variables that enter energy choices. And they have highlighted the role of technical expertise as a political resource employed by both sides of the nuclear debate to provide legitimacy and to reinforce existing beliefs.[8]

The confusion of information, evaluations, and conflicting claims that emerged from the Three

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[5] For general discussion of problems of coordinating interorganizational relationships, see McEachron and Tiege (1977).

[6] Since 1975, licensed power plant operators have been legally required to make agreements with state and local authorities about emergency plans, but as of March 1979, only 10 of 43 states with nuclear facilities had emergency response plans that met NRC specifications.

[7] For analysis of these problems with respect to environmental disputes, see DiMento (1977).

[8] For analysis of the political role of scientists in disputes, see Nelkin (1975).

Mile Island accident had just such effects. Hundreds of scientists from the NRC and other federal and state agencies, and utilities, and the industry, descended on Three Mile Island. In addition, "critical scientists" representing the nuclear opposition entered the debate. For example, a consulting firm had been formed by three General Electric nuclear engineers who had resigned from the company in 1976 in protest against what they felt was inadequate attention to nuclear safety and insoluble problems of human error associated with nuclear power. These scientists work for the anti-nuclear movement and were the technical consultants for the movie, *The China Syndrome,* released just prior to the accident and coincidentally portraying a very similar accident.

As these scientists assessed the problem, its dimensions and its causes were as varied as their interests and their concerns about the future of nuclear policy. The genuine technical uncertainty involved in evaluating risk allowed them a very wide range of interpretation. With a few standards to guide evaluation, scientists could easily make different judgments about the seriousness of health and environmental risks. Predictably, these judgments corresponded to these scientists' prior attitudes towards nuclear power (Nelkin, 1980).

Scientists at Three Mile Island were far more than simply investigators seeking the causes and dimensions of the accident. They served as publicists, image makers, as sources of credible information, as contacts for the persistent and ever-present press. They served to "represent" the views of both pro- and anti-nuclear groups, all with intense concerns about the future of nuclear power. Their public disputes in this and indeed in other controversies, creates a striking paradox. While inevitably undermining the credibility of scientific expertise as an objective basis for policy, public conflict has vastly expanded the expert's policy role. Special studies, commissions, and review boards have proliferated. Over $10 million was spent on official Three Mile Island inquiries, and hundreds of contracts for research on the causes and impacts of the accident were awarded.

The scientists' expanding influence following the Three Mile Island accident suggests the heavy dependence on the judgment of an expert elite whenever problems of technological risk arise. For power tends to gravitate towards those who can manage the critical information. As problems of risk assume increased policy importance, this tendency to augment the power of expertise raises serious questions about political legitimacy, exacerbating existing tensions over democratic control of technology and deepening public distrust (Kasperson, 1979).

The Media. The accident also had important implications for the media. The nuclear debate has increasingly engaged the press since the 1970s. Print media coverage of nuclear power increased by 400 percent between 1972 and 1976 (Rubin, 1979). But at Three Mile Island, the complex technical character of the events and the factual uncertainty among experts and regulatory officials came into conflict with enormous pressures for immediate reporting, to create a hopelessly confused situation. The public's right to information and the problems of communicating information about technology became the focus of a subsequent investigation.

The task force from the Kemeny Commission assessed these problems and reviewed the ability of the media to present timely, accurate and understandable information about the rapidly changing situation (Rubin, 1979). It found a number of basic problems. At least 500 journalists came to Three Mile Island. At first there existed no central information facilities, and no one to answer basic technical questions. The NRC also lacked the organization to deal effectively with public relations during the crisis. The utility was cautious and lacking in candor. With its reputation on the line, Metropolitan Edison "showed a marked capacity for self-deception [or] it hid behind technical jargon to obscure answers to troublesome questions" (Rubin, 1979, p. 9). Eventually the utility delegated the communication of official technical data to the NRC and avoided public interaction. However, it hired a team of public relations experts to present its views to the community.

Many reporters were poorly informed about rudimentary details of nuclear power, and this problem was compounded by the obfuscation caused by the technical language in which details were presented. Even those scientists who were willing to openly discuss the problems with reporters often had difficulties explaining themselves in non-technical terms. The problem, claimed the Kemeny Commission task force, was less the sensationalism of the press than the nature of the information actually provided to journalists—information often misleading and distorted by deliberate cover-ups and suppression of facts—as well as by genuine technical uncertainty.

In this context, press coverage was often confused, inaccurate, and indeed, as a consequence of inadequate information, sensationalist. The experience created an awareness of the importance of accurate communication—the need for journalists to develop some knowledge about nuclear technology, and especially for the nuclear establishment to provide a more candid and prompt response to demands for news.

Conclusions

Proponents and critics of nuclear power clearly view the risks of this technology through very different lenses. To those convinced of the importance of the nuclear program, the fact that the accident fell short of a meltdown demonstrated that the reactor safety systems basically worked, and that the public need only be reassured of their reliability. To critics, the accident revealed the potentially catastrophic risks of nuclear technology, but it also indicated serious political, psychological, and institutional ramifications that are neither easily measured nor resolved. As economic priorities and energy problems create powerful pressures for technological advancement, these less immediate and often vague social impacts of technology have tended to be dismissed.

I have used the Three Mile Island accident as an example through which to suggest a series of major institutional and policy issues surrounding complex technologies. The very scale of technological commitments; their complexity and the nature of their risks, generate their own institutional reality—a reality that calls for social science research. The area of regulation of technology is the most obvious link between technology and social science. Indeed, the problem of the NRC as both a promoter and regulator of nuclear power are familiar in the context of regulatory politics. But special problems arise when the stakes are very high. What kind of structures can be organized to enhance the accountability of this regulatory system, and the controllability of the nuclear industry with its monopoly of expertise? How does this industry deal with risk and the increasing pressure for greater public accountability?

The rapid increase in litigation as a means to force accountability presents a number of other problems suggested by the aftermath of Three Mile Island. The courts are poorly equipped to deal with the esoteric and uncertain scientific information that confounds the nuclear debate. Judges are accustomed to ruling on precedent, not on statistical assessments of potential risks. And they are not trained to evaluate the highly technical arguments that are increasingly involved in recent techno-political disputes. Moreover, many technologies raise problems that require reanalysis of legal principles: what is the appropriate role of law in assessing long-term effects of an accident such as Three Mile Island? How can courts be expected to have the competence to deal with technical issues? And how can they deal with questions of fairness when technological developments justified by the "public interest" infringe on local community interests? Who is liable for major accidents? Who is responsible for the long-term social costs that may be incurred?

The psychological stress observed in the Harrisburg region suggests a number of questions about the sociology of risk perception. What social processes are involved in the perception of risk by different groups? What are the discrepancies between perception and reality? How are these perceptions influenced by factors such as the availability of choice, the credibility of responsible institutions or the prevailing state of technical uncertainty?

Disputes among scientists not only affect public policy, but the very institution of science as well. What is the effect of such disputes on the scientific community and its traditional image of neutrality? On the role of science as a legitimating institution? What is the impact of technical uncertainty on the possibilities of public participation? On the credibility of policy-making institutions?

Finally, in the age of media politics, the role of the press looms large. Who speaks for government in communicating with the media? How can journalists develop the competence to deal accurately with events such as the Three Mile Island accident?

These are but a few of the many questions that have emerged from a social analysis of the Three Mile Island accident. It must be emphasized that social research on this event was an afterthought —the results of efforts by a small group of people who decided to collaborate in calling attention to the sociological ramifications of this event (Wolf, 1979). Eventually, however, the Kemeny Commission discovered that the accident was indeed a "people problem" that derived from the predispositions, the "mind sets," the myths of rationality and control held by key participants who tended consistently to dismiss the importance of human factors in managing nuclear technology:

> The Commission's attention slowly but steadily shifted almost completely from equipment to people. And we found that this was one of the most horrendous "people problems" we had ever encountered (Kemeny, 1980, p. 81).

Eventually, too, the problem was conceived less as a technical than a *social* system failure that derived in part from the nature of social relationships among the leading actors in the nuclear enterprise (Sills, 1980). In this sense, the Three Mile Island accident revealed with striking clarity that technology is a social concept to be understood in terms of social and political relationships if it is to be brought under public control.

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A Comment on Dorothy Nelkin's "Some Social and Political Dimensions of Nuclear Power: Examples from Three Mile Island"

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Dorothy Nelkin has written an informed, comprehensive, and useful summary of the diverse social and political aspects of the accident at the Three Mile Island nuclear power plant, and I have few quarrels with her conclusions. This agreement will come as no surprise to those who know that Nelkin and I have been closely associated on a number of nuclear power-related social science activities, including two on Three Mile Island (TMI) itself. The first such activity was a workshop on the social science aspects of TMI which we organized under the auspices of the Social Science Research Council, with funds from the President's Commission on the Accident at Three Mile Island (see Wolf, 1979); the papers presented at the workshop are to be published in early 1981 (Sills, Wolf, and Shelanski, forthcoming).

The second of these activities was a conference on the lessons and implications of TMI, held in April 1980. Sponsored by the New York Academy of Sciences and co-chaired by Thomas H. Moss and myself, the conference brought government officials and industry executives who had been participants in the post-TMI events together with natural and social scientists who had studied the accident. Nelkin was one of the major speakers. This is the first opportunity we have had to speak directly to the political science community about the social and political meaning of the accident at TMI.

Nelkin's article concentrates upon substance, upon those social and political dimensions of TMI that should interest political scientists. The paper is a model of substance, in fact, and it never raises the difficult question of why social scientists are asked to comment on "aspects," or "effects," or "dimensions," or "implications" of energy, rather than on energy itself. "Energy," I believe, is the product of social, economic, and political processes that define certain raw materials (coal, uranium, sunshine, wind) as resources. According to this formulation, "energy" is more of a social science concept than a physical science one.

The subject of my brief comment is thus more epistemological than it is substantive. My method, I fear, is polemical—in the theological sense of refuting what I take to be errors of doctrine.

The major doctrinal error that I wish to refute is the implicit or explicit assumption that energy is a technical subject to be studied by natural scientists. (This is how "energy research" is defined by the U.S. Department of Energy.) A corollary of this assumption is that social scientists should study not energy but the "implications" of energy, or the "effects" of energy on society. Because the issue is framed in this way, social scientists are usually brought in after the project is under way and the study design has been set. Sometimes a more explicit practice called "exclusion" is followed. Under this practice, the study directors acknowledge the importance of social and political factors, but deliberately exclude them as being beyond the "scope" of the study. An example is the MIT Energy Self-Sufficiency Study, as noted in the Harvard Business School Energy Project (Koreisha and Stobaugh, 1979, pp. 263-64).

A classic example is the well-known project of the National Academy of Sciences, the Committee on Nuclear and Alternate Energy Sources (CONAES), a project on which Nelkin and I both worked. More than a year had elapsed after CONAES was formed before our Working Group on Sociopolitical Effects was established. We worked hard and eventually published a report, but it is doubtful whether it had much impact. A prominent sociologist on the parent committee resigned more or less in disgust before the CONAES report was published.

Consider also the President's Commission on the Accident at Three Mile Island. In its report—which I generally admire—the commission stated that a principal cause of the accident was the "mindset" of personnel in the nuclear industry. What the report did not say is that the very same "mindset" that led to the accident also influenced the way the commission went about its task and recruited its staff. As the commission organized itself, it initially adopted a model of the accident that assumed it was an engineering failure that led to a plant shutdown and the release of radiation. The point was to find out who was responsible.

Because of this interpretation of the accident, the majority of the commission's staff consisted of nuclear physicists and engineers, accident analysts on leave from NASA, radiologists, lawyers, and journalists. Initially, little thought was given to the problems of organization and management, to say nothing of social behavior.

Interestingly, however, the final conclusions in the commission's report have very little to do with this interpretation; rather than its being an engineering failure, the commission concluded that the accident was the result of a social systems failure, although it did not use this term.

A view of the accident as primarily a social systems failure gained prominence in the commission's thinking during its investigation. According to its report, "As the evidence accumulated, it became clear that the fundamental problems are people-related problems and not equipment problems" (President's Commission, 1979, p. 8). John Kemeny, the chairman, noted in a 1980 speech at the Massachusetts Institute of Technology that "The Commission's attention slowly but steadily shifted, almost completely, from equipment to people. And we found that this was one of the most horrendous 'people problems' we had ever encountered" (p. 67).

The commission found that the crux of the "people problems" was in the perceptions and predispositions of operators, managers, vendors, and regulators. As the commission report stated, "In the testimony we received, one word occurred over and over again. That word is 'mindset' " (p. 8).

The attitudinal and ideological components of the word "mindset" are easily recognizable in the intellectual and research traditions of the social sciences. In psychology, there is the concept of the "response set"; in sociology, there are "dispositions"; in anthropology, there is "culturally determined"; and in economics, "propensity."

In sociotechnical systems such as nuclear power reactors, operators and equipment are of course not readily separable. The commission pointed to their inappropriate isolation from each other as a root cause of the accident:

> The most serious "mindset" is the preoccupation of everyone with the safety of equipment, resulting in the down-playing of the importance of the human element in nuclear power generation. We are tempted to say that while an enormous effort was expended to assure that safety-related equipment functioned as well as possible, and that there was backup equipment in depth, what the NRC [Nuclear Regulatory Commission] and the industry have failed to recognize sufficiently is that the human beings who manage and operate the plants constitute an important safety system (p. 10).

The NRC commissioned its own investigation of the accident by a special inquiry group directed by a Washington lawyer, Mitchell Rogovin. In its January 1980 report, the group also concluded that "the principal deficiencies in commercial reactor safety today are not hardware problems, they are management problems . . . problems [that] cannot be solved by the addition of a few pipes and valves—or, for that matter, by a resident Federal inspector" (Rogovin, 1980, Vol. 1, p. 89).

It would be pleasant to assume that this rather dramatic "discovery" by non-social scientists that the accident at TMI was a social science event would have an impact upon the nuclear energy establishment. It would be pleasant, but it hasn't happened. And the reason of course is that mindsets are not changed easily, particularly when history, career patterns, funding, and prestige are all on the side of considering nuclear energy as the nearly exclusive terrain of natural science and engineering.

It is obvious that nuclear fission is a subject for physicists to study, but nuclear energy—and, more broadly, energy from any source—is only in a relatively trivial sense a problem for physical scientists and engineers. In a technical sense it would be easy to produce adequate supplies of energy: both the popular and the scientific press report many schemes that would provide "unlimited" energy. The problem is not the techniques for producing energy; rather, it is that of producing it from raw materials that are owned by others (Arabs, American Indians, farmers) at a political and economic price we are willing to pay, making it widely accessible in socially acceptable forms, and avoiding its negative impacts. In short, energy is in large part a problem of social and political organization, and thus a problem for the social sciences.

The accident at TMI provides a dramatic example of the social nature of nuclear energy. When the Atomic Energy Commission commissioned the $3,000,000 Rasmussen Report (1974), it undoubtedly thought that it had taken a giant step forward in allaying public fears of accidents at nuclear power plants, since the report concluded that nuclear energy was much safer to produce than other forms of energy. However, the response to TMI demonstrated how little public perception of risk has to do with the kinds of calculations presented in this report. Risk analysis involves far more than the computation of probabilities. Individual and cultural variation must be considered, as must the enormous difference between risks that are taken for granted versus those that seem exogenous to a person's normal pattern of life.

Defining energy as a social rather than a physical concept will not take place this year or next: there is too much at stake. But there are several signs of hope. Social scientists themselves are beginning to study energy and as recent bibliographies (e.g., Frankena et al., 1979) and survey articles (e.g., Wilbanks, 1980) demonstrate, the interest of social scientists in these problems is spreading at a fairly rapid rate. Time is also on our side: the generation of physicists and engineers which developed nuclear weapons and nuclear power in the 1940s and 1950s is aging, and is being replaced in part by a generation that has been forced to acknowledge the social nature of energy. Although the social science community is not adequately prepared either to prevent or to study the "next" nuclear accident, it is much better prepared than ever before. Nelkin's article may thus be viewed both as the apogee of a concern for the social science "aspects" of nuclear energy and the harbinger of a time when these aspects will constitute the core (pun intended) of nuclear energy research.

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COMMUNICATIONS

ON ARTICLES

Comment on Smith (Vol. 74, September 1980, pp. 685-96)

TO THE EDITOR:

The ideological sophistication of the American electorate has increased during the postwar years. This finding has been reported in studies that analyzed the coded responses to the eight basic open-ended University of Michigan Survey Research Center questions about parties and candidates used to measure levels of conceptualization (Field and Anderson, 1969; Nie, Verba, and Petrocik, 1976, pp. 110-22), and has also been reported in studies that analyzed the verbatim responses to these same questions (Klingemann and Wright, 1973; Miller and Miller, 1976, pp. 844-45; Pierce and Hagner, 1980, pp. 72-73). Norman H. Nie, Sidney Verba, and John R. Petrocik provide the most comprehensive of these analyses, for they examined all six presidential elections from 1952 through 1972, and have now studied all seven elections through 1976.

In his recent article, Eric R. A. N. Smith (1980) calls all these studies into question. Smith analyzes the coded responses for the 1956-58-60 SRC panel data to demonstrate that among individuals, levels of conceptualization as measured in 1960 are only weakly related to these levels as measured in 1956. He argues that since level of conceptualization is supposed to measure how people think about politics, it should be a fairly stable attitude trait. Low over-time correlations, he argues, suggest that the party and candidate questions measure "the *rhetoric* of contemporary political discourse rather than the actual *process* of political evaluation" (p. 685). "The measures of the levels of conceptualization," he claims, "seem to be tapping the language that the evaluations take after they have been formed, rather than the actual processes of evaluation" (p. 694).

Smith raises some compelling logical arguments, especially when he discusses the processes through which people learn to evaluate parties and candidates. Yet his empirical findings are not conclusive, as he himself realizes. In fact, Smith's most important analysis, a three-wave reliability estimation of the party component of levels of conceptualization that employs the Nie, Verba, and Petrocik measure, may raise some doubts about his own conclusions. He reports that "virtually *all* of the change in the observed measures over the 1956-1960 period is due to measurement error. The underlying trait is nearly constant" (p. 693). In view of these findings, proponents of the conventional interpretation of these measures may be forced to concede that the Nie, Verba, Petrocik measure is unreliable. But they could argue that, once this measurement unreliability is taken into account, the "true" over-time stability of levels of conceptualization is high, precisely what one would expect if these procedures measured the way people think about politics.

Although Smith rejects the conventional interpretation of these measures, he accepts the conclusion that there has been a change in overall "levels of conceptualization," claiming that this means only that there has been an increase in the use of ideological rhetoric. Even if we accept Smith's basic argument, we might still want to know the extent to which the use of such language has changed during the postwar years.

The logical place to turn for the most complete analysis of change is the "enlarged" edition of *The Changing American Voter*. However, the newer edition raises serious problems. Although Nie, Verba, and Petrocik (1979) claim that levels of conceptualization declined in 1976, this conclusion is questionable. Moreover, there are major, unexplained differences between levels of conceptualization as reported in the 1976 edition of *The Changing American Voter* and those reported in the 1979 edition.

Let us begin with the Nie, Verba, and Petrocik

I am grateful to John H. Aldrich, Bernard M. Finifter, John E. Hunter, Frank A. Pinner, Charles O. Press, Brian D. Silver, and especially Ada W. Finifter, for suggestions on earlier versions of this comment. Two anonymous referees for the *Review* made helpful suggestions. The data used to attempt to replicate the Nie, Verba, and Petrocik results were provided by the Inter-University Consortium for Political and Social Research, which is not responsible for my analyses or interpretations. Rick E. Rollenhagen assisted with these data analyses.

claim that levels of conceptualization declined in 1976 (see 1979, pp. 365-67). In reporting a downturn, Nie and his colleagues state that they had anticipated a decline, given the nature of the 1976 election. As they write, "In the absence of a polarizing choice in 1976, we would expect the public to return to earlier models of thinking, to conceive of the candidates less in issue and ideological terms and to have less coherent issue positions" (1979, p. 365).

It is questionable whether levels of conceptualization actually declined in 1976. Let us recall that Campbell and his colleagues (1960, pp. 222-23) based their measure of levels of conceptualization upon the responses to eight open-ended questions about political parties and presidential candidates. Nie and his colleagues built three measures of levels of conceptualization, one based upon responses to the four party questions, one to the four candidate questions, and one that combined responses to all eight questions. Only the last measure can be compared with the reports of scholars who have used the verbatim responses. Though there may well be advantages to dividing levels of conceptualization into a separate party and candidate component, and although analyzing the party component separately was essential for Smith's three-wave panel analysis, the combined trend line becomes the most important to follow.

Unfortunately, Nie and his colleagues do not report these results for 1976. They report (1979, pp. 366-67) the percentage of ideologues for the candidate questions (7 percent), the total percentage of ideologues and near-ideologues for the candidate questions (16 percent), and the percentage of ideologues for the party questions (35 percent). Given these results, we know, by definition, that the *combined* percentage of ideologues for the party and candidate questions must be at least 35 percent. Yet the combined percentage of ideologues for the party and candidate questions in 1972 was only 33 percent (1976, p. 115). The claim that levels of conceptualization declined in 1976 is therefore valid for only one method of measuring these levels, and is not valid for the method (combining party and candidate responses) originally advanced in *The American Voter,* and which is used in much of Nie's, Verba's, and Petrocik's own analysis.

But just what was the percentage of ideologues in 1972? That depends on which edition of *The Changing American Voter* one refers to. Although we are told that the 1979 version of the book remains the same as the 1976 version (1979, p. viii), except for the added chapter on the 1976 election, and although we are provided with neither a warning nor an explanation, the percentage of ideologues differs in the 1979 (p. 115) and the 1976 (p. 115) editions of the book. In fact, Figure 7.3, which reports the total percentage of ideologues and near-ideologues, has been replaced by a new Figure 7.3. Figure 1 of this comment presents the percentage of ideologues and of ideologues plus near-ideologues in the two editions of the book. For each measure, the solid line shows the results as published in 1976, the broken line as they were published in 1979. As Figure 1 reveals, there are differences between the results for ideologues for all years except 1956. For the combined percentage of ideologues and near-ideologues, the results are always different, and are dramatically different for 1964 and 1968.

Which version of the data should we accept? As there is no discussion of the newly reported results, Nie, Verba, and Petrocik provide us with no guidance. Nor can available survey data be used easily to choose between the two sets of results, since the published coding procedures (1976, pp. 373-75; 1979, pp. 403-405) are applicable only to the party questions. As these procedures provide no warning that the categories are applicable only to the party questions, anyone who directly followed them would inadvertently be applying a meaningless schema to the four candidate questions.[1]

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[1] I did use the available SRC data for 1964 and 1968 to attempt to determine which set of figures was more nearly correct. By following the procedures spelled out in Appendix 2C of *The Changing American Voter,* I came close to replicating the total percentage of ideologues and near-ideologues reported in the 1976 edition. However, a close examination of ICPSR 1964 and 1968 election codebooks (as well as those for 1952, 1956, and 1960) reveals that this coding schema is, in fact, applicable only to the party questions, because the same code numbers that refer to ideological type *party* references frequently refer to nonideological type *candidate* references. (For example, value "270" for the party questions refers to, "Like their liberal philosophy," or "Don't like their liberal philosophy," while the same value for the candidate questions refers to "Good family life; like his family," or "Don't like his family.") I then used my own coding decisions to attempt to build a measure of ideology and near-ideology for the candidate questions, and by using my own procedures (along with *The Changing American Voter* procedures for the party questions) came close to reproducing the results of the new Figure 7.3. However, in no case could I exactly reproduce the results reported in either version of Figure 7.3. Moreover, although I derived exactly the same results for the party questions reported in Figure 7.1, I could not reproduce the results for the candidate questions reported in Figure 7.2 of *The Changing American Voter.* In fact, so many coding decisions are necessary to create a coding schema for the candidate questions that it would be difficult to assure that any newly constructed schema would be comparable to that used by Nie, Verba, and Petrocik.

Whether these problems bear upon Smith's analysis is difficult to say. Smith reports (p. 687, n. 4) that "a complete description of the measure" is found on pp. 18-23 and in Appendix 2C of *The Changing American Voter.* It is possible, however, that Smith did not follow the Nie, Verba, and Petrocik procedures, since the percentage of ideologues and of near-ideologues in his analysis (see Table 3, p. 689) differs substantially from the percentages reported in either edition of *The Changing American Voter.*[2] Even if he did use the published Nie, Verba, and Petrocik procedures, this error would not affect his analysis of the levels of conceptualization measures developed by Field and Anderson, or of the party component measure based upon the Nie, Verba, and Petrocik schema. Fortunately, Smith's three-wave panel analysis is based only on the party component.

Despite the difficulty in replicating the Nie, Verba, and Petrocik results, it seems reasonable to assume that the original *Changing American Voter* report was wrong, and that the newly published figures are more nearly correct. How else can we explain the fact that a new figure was substituted for the old?[3]

If the newly reported results are correct, the problems are more far-reaching than a mere discrepancy between one edition and its successor, since the newly reported results do not correspond with the Nie, Verba, and Petrocik interpretation of changing levels of conceptualization. Rather than increasing between 1960 (the nonideological Kennedy-Nixon contest) and 1964 (with Goldwater's explicitly ideological appeals), the percentage of ideologues marginally declines. Rather than a dramatic 18-percentage-point jump in ideologues and near-ideologues, there is a negligible 3-percentage-point increase. As opposed to the dramatic step-function increase reported in the 1976 edition, we now see a far more gradual rise in the percentage of ideologues and near-

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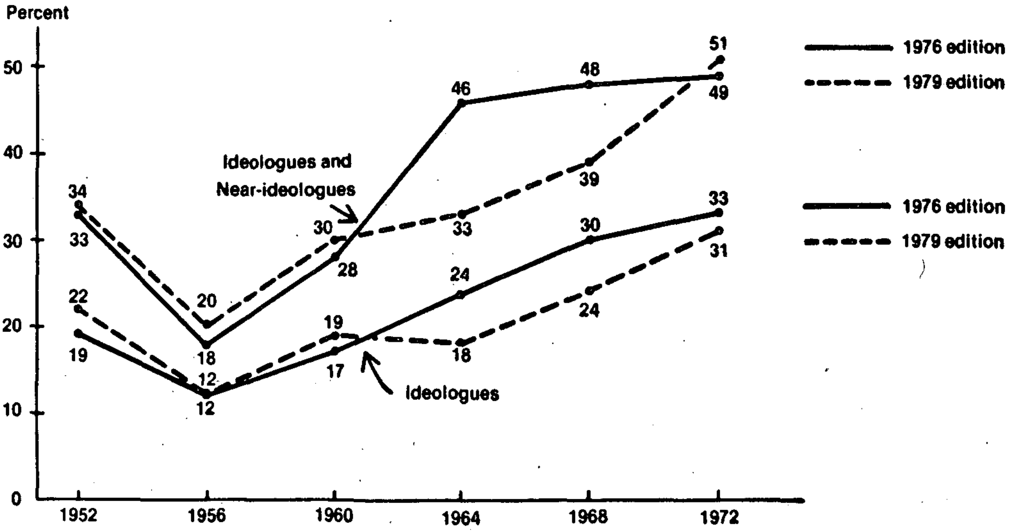
[2] One would expect some differences between Smith's results and those reported in *The Changing American Voter,* since Smith's table is based only upon respondents in both the 1956 and 1960 waves of the SRC panel, while Nie, Verba, and Petrocik's analysis is based upon the 1956 and 1960 electorate samples. But the differences between Smith's results and those reported by Nie and his colleagues are greater than one might expect from systematic biases caused by panel attrition.

[3] Assuming that the authors have corrected an earlier error, they are certainly to be commended. It would have been helpful, however, if they had explained the reason for the change, and the implications of this change for other figures reported in their chapter on changing levels of conceptualization.



Sources: Solid lines based on Norman H. Nie, Sidney Verba, and John R. Petrocik, *The Changing American Voter* (Cambridge, Mass., Harvard University Press, 1976), p. 115. Broken lines based on *The Changing American Voter,* enlarged edition (1979), p. 115.

Figure 1. Ideological Evaluation of Candidates and Parties from 1952 through 1972 (Percentage of Ideologues and of Ideologues and Near-Ideologues in the Two Editions of *The Changing American Voter*)

ideologues. In short, the processes that Nie and his colleagues claim led to the increase in levels of conceptualization do not correspond to the data as reported in the 1979 edition. Overall levels of conceptualization do not appear to respond to short-term political conditions in the way Nie, Verba, and Petrocik claim.

A comprehensive search for the causes of changing levels of conceptualization poses interesting research problems. The newly reported trend line suggests that rising levels of conceptualization may result from long-term secular changes. For example, increases in education among the electorate may be more important in creating these changes than Nie and his colleagues claimed. Perhaps television, by lowering the cost of political information, has led to gradual increases in ideological sophistication. Perhaps, too, learning to become ideologically sophisticated is a long-term process, which is reinforced over a series of ideological elections, or by living in a more polarized political era. Exploring these possibilities is a major research project that could yield important methodological insights by shedding light on the robustness of these measures in the face of alternative coding procedures. Such research might or might not contradict the main thesis of *The Changing American Voter,* but, regardless of the outcome, would give us a more solid understanding of the processes through which levels of conceptualization change over time.

If we accept Smith's basic thesis, such an analysis would be less important. Even so, we might wish to understand the processes through which the use of ideological rhetoric increased during the postwar years, for this increase may reflect real-world change in political behavior. Once again, we would need better research designs to explain change more adequately.

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Reply

TO THE EDITOR:

Paul R. Abramson (1981) and Eric R. A. N. Smith (1980) both comment on the "levels of conceptualization" measure in *The Changing American Voter.*

First to Abramson: He has indeed caught a large discrepancy between the original edition of *The Changing American Voter* and the enlarged edition. The new data would weaken our argument as to a shift in 1964. Abramson surmises that we corrected the data when we prepared the enlarged edition and quite properly suggests that we should, under the circumstances, have made some comment.

In fact, the story is somewhat different, but almost as embarrassing. The enlarged edition differs from the earlier edition only in the addition of a new chapter on the 1976 election. The earlier chapters are unchanged. We did, however, take the opportunity to correct typographical errors in the text and data. In the case of Figure 7.3, there were some minor changes to be made (the percentage of ideologues in 1972 should be 45 percent rather than 49 percent as in the original edition; the combined percentage of ideologues and near-ideologues should be 23 percent instead of 24 percent in 1964). Rather than make those minor changes, we substituted completely incorrect data in the enlarged edition. In short, Figure 7.3 in the original edition is correct; the new enlarged edition is wrong in that respect. The appended Figure 1 contains the correct data for the combined party and candidate measure which should replace the marginally incorrect Figure 7.3 in the original edition as well as the grossly mistaken Figure 7.3 of the enlarged edition. Figure 1 also brings the data series up through 1976.

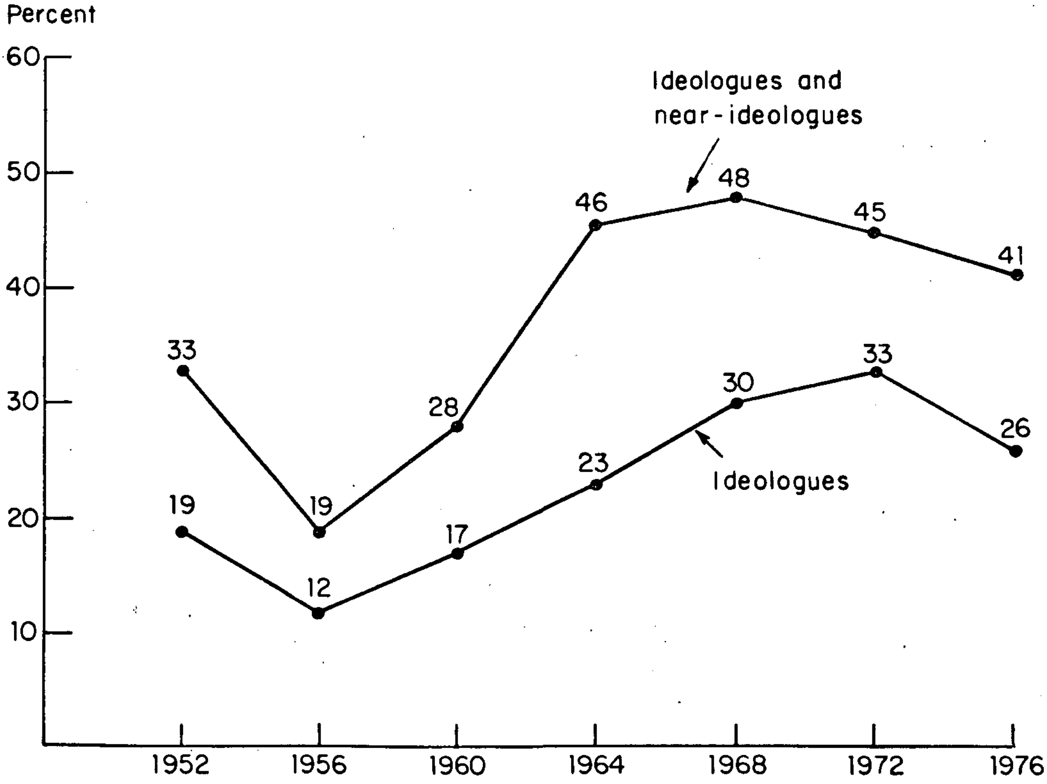
We are grateful to Abramson for calling the error to our attention.

How could it happen? you may ask. The specific mistake was a clerical confusion among several variables with SPSS acronyms that were almost identical (users beware!). It was—as it almost always is—human error, not machine error. And unfortunately, we were the humans.

The same clerical confusion led to another mistake which we would like to correct as well. It relates to another point that Abramson makes. On page 366 of the enlarged edition, we report that "the proportion of ideologues vis-à-vis parties rises from 22 percent in 1972 to 35 percent in 1976." The correct figure for 1976 should be 24 percent. This figure is consistent with a decline in the combined candidate/party measure in 1976.

To summarize, now that we have got the data correct: There was an increase in the proportion of respondents using ideological or near-ideological terms between 1960 and 1964. However, the increase on the combined candidate and party measure derives, as we point out in *The Changing American Voter,* almost entirely from a change in the terms used to refer to candidates. The party references change very little. The situation is similar in 1976. There is a sharp drop in ideological and near-ideological references to candidates but almost no change (indeed, a slight increase) in such references to political parties. All this is consistent with the argument made in *The Changing American Voter* that respondents are sensitive to the stimuli they are offered. The candidate rather than the party stimuli change from election to election.

This brings us to Eric R. A. N. Smith's article, "The Levels of Conceptualization: False Measures of Ideological Sophistication." Smith analyzes our measure of "levels of conceptualization" based on categories of the CPS master codes. He concludes that "instead of reflecting conceptual sophistication, the measures are reflecting short-term forces unrelated to conceptual sophistication" (p. 674). "They do not and can-



Sources: Norman H. Nie, Sidney Verba, and John R. Petrocik, *The Changing American Voter,* enlarged edition (Cambridge, Mass.: Harvard University Press, 1979), p. 114, Figure 7.3 (as corrected in this communication); Eric R. A. N. Smith, "The Levels of Conceptualization: False Measures of Ideological Sophistication," *American Political Science Review* 74 (1980), p. 689, Table 3.

Figure 1. Corrected Combined Ideologue/Near-Ideologue Distribution, 1952-1976

not show that the electorate is becoming more ideologically aware" (p. 676). This is parallel to our own conclusion. As we wrote in the original edition of *The Changing American Voter,*

> The data on levels of conceptualization do support the hypothesis that the way in which citizens conceptualize the political realm is dependent on the political content to which they are exposed (p. 121). To understand the sources of these changes [in attitude consistency and levels of conceptualization], it may be better to look outside of the individual citizen to the real world of politics. The way in which people think about the political world is not merely the result of their social and psychological characteristics—the education they have or their cognitive capacities. The way people think about politics is also a reflection of the stimuli offered them by the political world. . . . If greater political sophistication were the explanation, one would expect more frequent political reference among the upper educated and/or a greater change in that frequency among those citizens with higher levels of education. Rather the data suggest that the world of politics impinges on the lives of all citizens (pp. 151-52).

This conclusion in the original edition of *The Changing American Voter* is reinforced by the findings reported in the enlarged edition that attitude consistency and level of conceptualization both fell in 1976 from their 1972 levels. The explanation has to lie in the nature of political discourse in 1976, not in a sudden decline in political sophistication.

Smith obviously misreads us when he says we argue that the change in our measure indicates "real change in the level of conceptual sophistication" (p. 694). Our interpretation is quite consistent with Smith's argument that the measures represent responses to "contemporary discourse" in a particular election.

Not only is Smith's overall interpretation consistent with our own, but also his data are consistent with our own qualification of that conclusion: that there is evidence that the measure may pick up long-term "cognitive limitations, especially at the lower reaches of the levels of conceptualization scale" (p. 122). Smith's data (Table 3, p. 689) show that of those respondents categorized as "apolitical" in 1956 because they gave apolitical or nature-of-the-times responses, only 4 percent had risen to the near-ideologue category in 1960 and 1 percent to the ideologue. In comparison, about a third of those in the middle categories had risen to one of the upper two categories in 1960.

We also agree with Smith's strictures as to the value of the master codes for building measures that achieve any depth in exposing the degree of conceptual sophistication of the electorate. It is doubtful that the level of conceptualization concept, as Converse meant it, can be validly measured by the mechanical use of the open-ended master code. One probably has to read the individual protocols to distinguish between those people who use general concepts for nuanced and complex interpretations of the political process and those who have learned a number of "buzz words" to describe contemporary affairs. In short, an analysis of the level of conceptualization of the public using the master codes illustrates the responsiveness of the population to contemporary politics rather than some fundamental change in the sophistication with which they analyze political matters.

Where we disagree with Smith is in his finding that our measure of levels of conceptualization shows so much temporal instability as to be an unreliable measure of the terms which respondents use to evaluate candidates and parties. The reason we doubt the relevance of his results to our measure is that he is apparently using a quite different measure. Table 1 compares our level of conceptualization measure with Smith's for both the party and candidate measures in 1956 and 1960. In both years Smith finds many more ideologues and near-ideologues than we do. We cannot help but wonder if a large portion of the instability that he reports simply reflects the use of a measure that is less demanding than ours. If he has misclassified many respondents (and we are certain that he has identified as ideologues or near-ideologues many respondents who would not make it by our criteria, and certainly not by a closer content analysis), the low correlations in his tables may reflect the looser definition he is using.[1] The difference between our measure and Smith's means that one cannot use his research to evaluate our analysis.

Lastly, even if the levels measure used by Smith

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[1] We may be partially to blame for this discrepancy. The codes presented in Appendix 2-C of *The Changing American Voter* are valid for creating a level of conceptualization measure applicable to political parties. Somewhat different codes have to be used for candidates. (In addition, there are some year-by-year modifications that must be carried out.) However, several years ago we sent Smith a copy of the full SPSS code used to build the two separate measures. In a recent letter to John Petrocik, Smith indicated he was having difficulty replicating our findings. We were not quite sure why and sent another copy of the code. In any case, the letter (dated July 31, 1980) arrived after Smith's article was in page proof and was too late to have any effect on the material he has published.

The categorization using the master code requires adjustments in 1968 and 1972. Those who want copies of the code needed to create the categorization, including these adjustments, should contact one of the authors.

Table 1. Comparison of Smith's Combined Level of Conceptualization Measure with Ours (Percent)

	Ideologues	
	Smith	*The Changing American Voter*
1956	24	12
1960	31	17
	Ideologues and Near-Ideologues	
	Smith	*The Changing American Voter*
1956	37	19
1960	53	28

Sources: Norman H. Nie, Sidney Verba, and John R. Petrocik, *The Changing American Voter*, enlarged edition (Cambridge, Mass.: Harvard University Press, 1979), p. 114, Figure 7.3 (as corrected in this communication); Eric R. A. N. Smith, "The Levels of Conceptualization: False Measures of Ideological Sophistication," *American Political Science Review* 74 (1980), p. 689, Table 3.

were the same as our own, we are not sure that one can justify Smith's crucial test of the reliability versus stability of the measures. He uses the Wiley and Wiley technique on the three-wave 1956, 1958, and 1960 data. However, our measure does not meet the appropriate statistical criteria for such analysis. Smith believes our measure "approximates an interval measure" (p. 793, n. 11). We are sure it is not an interval measure and doubt that it even approximates one. The level of conceptualization typology is clearly multi-dimensional. There is perhaps an abstractness dimension separating ideologues from apoliticals; but we have no idea what dimension orders the other categories. If the levels are categorical (and we think they are) nothing is learned from the Wiley and Wiley technique, since it is inappropriate to perform such statistical computation on categories.

In sum, though we agree with the main thrust of Smith's interpretation of his findings, we do not believe that the measure he uses is comparable enough to our own to allow of close comparison, nor are we convinced by the statistical techniques he uses to analyze the measure.

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Reply

TO THE EDITOR:

The letters by Paul Abramson (1981) and by Norman Nie, Sidney Verba and John Petrocik (1981) raise interesting points which require comment. I shall address myself to Abramson's comments first.

Abramson discusses my analysis of the party component of the Nie et al. level-of-conceptualization measure in which the reliability of the measure and the stability of the underlying trait over time are estimated.[1] He notes that one interpretation of these data is that the measures are very unreliable, but that the underlying trait—conceptual sophistication—is very stable. If the estimates of reliability and stability were taken in isolation, ignoring the other available data, then such an interpretation might be reasonable. But other data are available, and these data lead to the conclusion that the underlying trait being measured is not level of conceptual sophistication. In other words, the data suggest that the measure is not valid.[2] Let us briefly review the construction of the level measures and then consider the evidence.

The level-of-conceptualization measures are based on eight open-ended questions asking what the respondent likes and dislikes about each of the major parties and their presidential candidates.

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[1] The reliability of a measure is the proportion of true score variance out of the total variance (which equals the true score variance plus the random error variance). The stability of a trait is the correlation of the true scores at two points in time.

[2] Loosely speaking, a measure is valid if it measures the phenomenon which it is claimed to measure. Validity is a matter of degree, expressed in a validity coefficient. More formally, "The most common way of explicating the general concept of validity in statistical terms is as a *validity coefficient,* the (absolute value of the) linear product moment correlation between the test and a specified criterion" (Lord and Novick, 1968, p. 261).

Campbell and his colleagues (1960), who create the original measure, believe that all eight questions tap the same underlying trait, namely, the level of sophistication at which people conceptualize politics. So they create a single measure based on the eight questions. When Nie, Verba and Petrocik deal with the measure, they first separate the questions on the parties from the questions on the candidates and construct two subscales, or "components." They then combine the two components into an overall measure.

Because the two components measure very closely related phenomena (if not, in fact, the same phenomenon), they should be highly correlated. The correlation coefficient between the two components is a test of validity, a validity coefficient. Looking at the data, we find that the correlation between the party and candidate components in 1956 was only .28. In 1960, it was .31. Both of these validity coefficients are extremely low, indicating that the two components are only weakly related, and that the overall measure is not valid.

When faced with findings such as these, we must ask why the validity coefficients should be so low. One explanation is that the spontaneous use of ideological rhetoric (e.g., words such as "liberal" and "conservative") in responding to the eight open-ended questions is a poor indicator of the level of sophistication at which people think about politics. This explanation is supported by findings by Field and Anderson (1969, p. 388) and Pierce (1970, p. 34) that the use of ideological rhetoric varies independently of concern for substantive ideological problems and knowledge about the meanings of the words "liberal" and "conservative." Thus it is not at all clear what people's use of ideological rhetoric tells us about how they think about politics. While, as Abramson points out, these data are not conclusive, they surely cast doubt on the validity of the level-of-conceptualization measures.

The second question raised by Abramson is whether I correctly replicate the *Changing American Voter* candidate component of the levels measure. Nie, Verba and Petrocik raise the same question and suggest that my findings are worthless because I did not replicate their work exactly. I did make a mistake in recreating their candidate component measure; however, the mistake was not large and the corrected data support my position more strongly than did the original, incorrect data.

Tables 1 and 2 present the corrected data. Table 1 shows a cross-tabulation of the overall level-of-conceptualization measure in 1960 with the same measure in 1956. The test-retest reliability coefficient of .31 differs by only .01 from the coefficient originally reported. Table 2 shows a cross-tabulation of the candidate component measure in 1960 with the same measure in 1956. The originally reported reliability coefficient was .20. The correct reliability coefficient is actually only .15. This is the only substantive difference from data presented in the article. It indicates that the candidate component measure was slightly less reliable than stated in the original article. Thus none of the conclusions based on these data is affected in any way.

The principal criticism by Nie and his colleagues is that my findings do no more than "parallel" what they say in *The Changing*

Table 1. The Nie, Verba and Petrocik Levels of Conceptualization, 1960 by 1956 (as Corrected)*

	1956								
1960	Apolitical	Party	Groups	Issues	Groups/ Issues	Near- Ideologues	Ideo- logues	Total	N
Apolitical	38%	10%	7%	8%	1%	5%	1%	8%	94
Party	17	22	14	16	6	9	5	13	158
Groups	16	13	26	10	11	9	9	14	163
Issues	18	28	19	32	23	23	9	23	273
Groups/Issues	6	9	14	12	31	7	10	14	170
Near-Ideologues	4	12	6	11	6	22	20	11	130
Ideologues	2	6	14	13	21	25	47	18	216
Total	7%	17%	17%	21%	18%	7%	13%	100%	
N	84	204	209	253	215	87	152		1204

Tau-b = .31

Source: Data from the 1956-1960 National Election Panel Study, Center for Political Studies, University of Michigan. John Petrocik provided instructions for coding.

*This ise the corrected version of Table 3, from Eric R. A. N. Smith, "The Levels of Conceptualization: False Measures of Ideological Sophistication," *American Political Science Review* 74 (1980), p. 689.

Table 2. The Nie, Verba and Petrocik Candidate Component, 1960 by 1956 (as Corrected)*

	1956								
1960	Apolitical	Party	Groups	Issues	Groups/ Issues	Near-Ideologues	Ideologues	Total	N
Apolitical	37%	12%	13%	12%	12%	9%	13%	16%	194
Party	30	41	30	36	24	36	40	36	428
Groups	8	8	10	5	12	9	0	8	92
Issues	17	29	29	32	30	24	0	27	329
Groups/Issues	4	4	9	7	12	6	7	6	67
Near-Ideologues	3	5	6	5	4	6	20	5	58
Ideologues	2	2	3	3	7	9	20	3	36
Total	16%	40%	9%	25%	6%	3%	1%	100%	
N	190	483	107	300	76	33	15		1204

Tau-b = .15

Source: Data from the 1956-1960 National Election Panel Study, Center for Political Studies, University of Michigan. John Petrocik provided instructions for coding.

*This is the corrected version of Table 6B, from Eric R. A. N. Smith, "The Levels of Conceptualization: False Measures of Ideological Sophistication," *American Political Science Review* 74 (1980), p. 692.

American Voter. That is not the case. My position is that their level-of-conceptualization measures are neither reliable nor valid.

They deny this, claiming that their measures are both valid and reliable. But this implies that the conceptual sophistication of one's thinking about politics can change radically from one year to the next. Such a notion of conceptual sophistication is difficult to accept.

One problem with evaluating the claim of "parallel" findings is that it is not clear what Nie, Verba and Petrocik think their measures reflect. At one point, they argue that I have misread them when I say that change in their measure indicates "real change in the level of conceptual sophistication." Apparently they see some difference between the terms "level of conceptualization" and "conceptual sophistication." Whatever this difference is, they fail to explain it. Since I follow Campbell et al. (1960, p. 222) in treating the terms synonymously, I have no idea of how I might have misread them. In sum, they are not entirely clear about what they mean by the term "level of conceptualization."

Nie and his colleagues close by criticizing my use of the Wiley and Wiley technique for estimating reliability and stability. In doing so, they say that the level of conceptualization typology is "clearly multi-dimensional" and should be treated as a categorical variable. But if it so "clearly" multi-dimensional, what are some of the other dimensions? They have no idea. They present no evidence.

The interval-level assumption underlying my use of the Wiley and Wiley technique is obviously not fully met. But it is valuable because it allows us to approach the problem of the reliability of the measures from a second perspective. The test-retest reliability coefficients yield a finding of low reliability. The Wiley and Wiley technique, based on different assumptions and different data, yields the same answer. The conclusion that the measures are unreliable was further supported.

In conclusion, I think that the comments by Nie, Verba and Petrocik fail either to demonstrate that their measures are reliable and valid or to show that my findings are consistent with the analysis they present in *The Changing American Voter.*

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Comment on the Merelman-Jennings Exchange (Vol. 74, June 1980, pp. 319-41)

TO THE EDITOR:

Richard Merelman and I continue to have different perspectives regarding the description of the basic shape of schooling and the conflict between democratic values and the basic shape of schooling, these being the subjects of the last two sections of his "Reply" (1980, pp. 339-40). However, I think the relative merits of our not-so-serious differences can be safely judged by the readers without a point-for-point elaboration on my part.

A far more fundamental and less subjective set of differences involves the key issue of whether schooling contributes to the development of democratic values (1980, pp. 338-39). Merelman's first impulse is to dismiss as irrelevant my basic point about the widely observed relationship between education and democratic values among adults. It is irrelevant, he says, because he was talking about pre-adults. It puzzles me as to why the later effects of pre-adult education are not germane to his argument, since education is usually seen, both empirically and normatively, as preparation for later life. Indeed, I thought that was the whole point of Merelman's brief, for if education is not related in either a good or bad way to the quality of (adult) democratic life, then why bother talking about it?

In any event, Merelman is incorrect even with respect to pre-adults. If the numerous studies of pre-adult socialization have shown anything, it is that knowledge of and commitment to democratic values and practices undergo enormous progression over the pre-adult years. Of course the schools do not deserve sole credit for that accomplishment. But it is foolish to ignore this developmental profile in making grand assumptions about the inefficacy of schooling.

To compound his predicament, Merelman rejects his first impulse and decides to challenge my assertions about the salutary effects of education on adult values and the suggestive similarity between that nexus and the one between education and occupational attainment. He does so by referencing the literature on the association between education and political knowledge rather than democratic values. He would have been better off sticking to values, though even there the evidence is decidedly against him. In the first place, he casts the link between education and occupational attainment in a very generous light by citing some of the most handsome results ever obtained in that literature. Somewhat more modest figures, for example, are reported by Treiman (1977, pp. 208-13). It is also worth noting that education is often (and legitimately) used in estimating occupational status scores, as in the famous Duncan indexes, thus helping ensure vibrant relationships between the two.

Second, and much more seriously, Merelman severely understates the education-political knowledge link—as scholars who have worked in the area would instinctively suspect. He relies on correlations based on very gross, single-item indicators which, by the way, happened to be among the very few out of hundreds reported by Hyman, Wright, and Reed (1975) as product-moment rather than gamma coefficients. A little more sensitivity on this point would have alerted Merelman to the presence of several data sets which could have been used to demonstrate more accurately the strength of the education-knowledge relationship.

I ransacked a few large-scale American studies that contained knowledge questions. In some instances the investigators had already fashioned summary scores; in other instances I did so by generating simple additive indexes. The results are instructive.

1. 1960—Almond and Verba, *Civic Culture*, national cross section, number of cabinet positions; r = .51.
2. 1961—Matthews and Prothro, southern participation study, representative cross section of the South, six questions on current history and institutions; r = .49.
3. 1964—SRC/CPS election study, national cross section, four questions regarding elections; r = .41.
4. 1965—Jennings and Niemi, political socialization study, national cross section of parents, six questions on institutions, current and past political history; r = .48.
5. 1967—Verba and Nie, *Participation in America*, national cross section, five questions on names of state and local officials; r = .36.
6. 1970—SRC/CPS election study, national cross section, four questions on institutions and laws; r = .41.
7. 1972—SRC/CPS election study, national cross section, five questions on institutions and congressional elections; r = .38.
8. 1973—Jennings and Niemi, socialization

study, national sample of parents, same six questions as 1965; r = .48.

9. 1973—Jennings and Niemi, socialization study, national sample of 1965 high school seniors, same six questions as above; r = .48.
10. 1974—Barnes and Kaase, et al., *Political Action*, national cross section, four open-ended questions yielding levels of ideological thinking index (à la Converse); r = .42.

Now these results represent a quantum jump over those reported by Merelman. Moreover, some of them rest on questions dealing with and occurring in conjunction with national elections, surely a circumstance that, due to the activation effect of elections, serves to depress the relationship. Nor have most investigators devoted much effort at all to developing more reliable and valid indicators of knowledge. In light of these considerations and the strong findings, coupled with a slight deflation of the generous estimates regarding status attainment, the distance between the putative effects of schooling on political knowledge and on status attainment is spectacularly far less than we were led to believe. Therefore, I must again question the empirical basis upon which Merelman erects his argument. It is correspondingly specious to make much of the invidious comparisons rendered by the causal model he offers, which struck me as strained in any case.

Because our disagreement was over the development of values rather than knowledge, this discussion is slightly tangential. However, Merelman chose to develop a point on these grounds, so it became my task to consider that argument. But the connection to the values question is reasonably clear. Whether through socialization, certification, or both, schooling does contributue to the development of democratic values, just as it does to political knowledge. (Incidentally, the selective critique by Jackman, 1978, has already been questioned—see Corbett, 1980.) The question remains, and here I really think Merelman does agree with me, how does schooling work its effects? Given universal education, much homogeneity in the educational system, the widespread mass culture, and the simultaneous presences of other causal agents, answering this question is no mean task. But it seems to me a much more promising approach than making erroneous assumptions about no effects and then trying to figure out why no effects exist.

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Reply

TO THE EDITOR:

I am pleased that M. Kent Jennings should wish to continue our dialogue about my "Democratic Politics and the Culture of American Education." Jennings is one of the more talented survey analysts of our discipline, and his work on political socialization and the politics of education has been invaluable. Unfortunately, however, as with all good survey researchers, there is an occasional tendency to assume that numbers convey single, unambiguous meanings, a tendency which sometimes makes the analyst mistake the trees for the forest.

Jennings still apparently misunderstands my reluctance to discuss adult political values and information. My essay was directed towards primary and secondary schooling; the "basic shape of schooling" does not even apply to universities, much less to later years. It therefore seems to me that data gathered from children and adolescents are most helpful in addressing the questions I raised. Even these data are flawed, of course, but they present fewer problems of interpretation than do data on adults, for the passage of years makes interpretation of adult data in terms of presumed childhood and adolescent influences difficult. Obviously, Jennings is correct in arguing that adult attitudes and information do constitute the "payoff" for what I am talking about in primary and secondary schools, but I operated on the assumption that you learn more about what concerns you by looking directly at *it*, rather than by looking at something problematically related to it at some years' remove.

But in his communication Jennings contends that even in pre-adult years "knowledge and commitment to democratic values and practices undergo enormous progression. . . ." He cites no evidence for this contention, so I looked myself at data on age-related information change from childhood to late adolescence. (And information *is* more changeable than democratic values, as Jennings admits.) The record is mixed. My own cross-sectional study in Los Angeles (1971) reported a roughly 30 percent improvement from 6th to 12th grade in political information (Merelman, 1971, p. 85). Andrain (1971, p. 77) reports more movement (approximately 35 percent from 5th to 8th grade). In the fullest recent study (Torney, Oppenheim, and Farnen, 1975, p. 107) American students from 10 to 18 years of age were tested, and those from 14 to 18 improved an average of 23 percent in cognitive items; incidentally, and not surprisingly given my argument, this improvement was the lowest of any country examined in this comparative study. A brief perusal of cohort data—obviously more reliable—in the recent National Assessment of Educational Progress summary (1979, p. 22) suggests that on retested items from 1969 to 1976 there was considerable variability in growth. For example, in 1971-72 17 percent of the 13-year-olds knew how the president was nominated, as opposed to 36 percent of the 17-year-olds in 1975-76, an improvement of only 19 percent. Other items showed more improvement, although there is a clear decline in what the 13-year-olds of 1975-76 knew in comparison with what the 13-year-olds of 1969-70 knew. Ditto with 17-year-olds.

All we can really do is to estimate, and let us do so generously. Suppose we put the figure at a 40-percent improvement rate in political information from 6th to 12th grade. Impressive? Only if we ignore the fact that much of this improvement is not the school's doing. At least three other factors must be considered: (1) genetic and social maturation of every child's intellectual abilities, a process which the school affects only marginally; (2) exposure to information about politics from sources other than the school; (3) improved guessing abilities and practice effects. Again let us estimate the effect of these factors. My feeling is that they explain perhaps 3/8 of the variance, namely 15 percent. This estimate leaves us with a "true" 25-percent increase in political information from 6th to 12th grade that is attributable to schooling. This figure, in turn, works out to approximately a 4-percent improvement per year. Given the fact that social studies classes take up about one hour each school day during the school year (an average of 240 days), what we have in effect is the equivalent of six 40-hour weeks of social studies teaching increasing political information at a 4-percent clip per annum. I'm not sure I'd call that a good return on investment; perhaps Jennings would.

Jennings observes near the end of his communication how difficult it is to make inferences about school effects, given the homogeneity of the public school system. I couldn't agree more; indeed, one of the things I attempted to do in my piece was to explain why the system tended toward homogeneity in the first place, namely, because of our desire to teach political equality in school. We are certainly under no such injunction to practice equality in mathematics teaching, by contrast, where we find that ability grouping takes place early and rapidly becomes associated with differential achievement (Rakow, Airasian, and Madaus, 1978, pp. 15-21). If it is unacceptable to differentiate by ability in social studies teaching (as opposed to mathematics), surely we should inquire into the cultural and political reasons why this is so, rather than simply to lament the difficulty of interpreting the problematic numbers that emerge from this practice.

Jennings comes closer to the mark in his comments on the adult-level data. Nevertheless, he too appears to read fairly selectively on the subject. Contrary to his assertion, Treiman (1977) does not report a "more modest" correlation of education and status than I reported. Indeed, on page 115 of his book his "purer" status measure yields a net regression coefficient of .58 with educational attainment, only trivially different from the .60 I originally reported. And, while the educational attainment-political information correlations Jennings remarks for adults are now at least in the same ball game as the status-attainment correlation, they are still lower. Nor are they entirely persuasive. For one thing, they include the effects of college, which, as Hyman, Wright, and Reed point out (1975, p. 107), importantly improves the ability to deal with complicated ideas. They also include the motivational consequences of differing levels of educational attainment. In a society which so values "education" (at least as measured in years, if not in quality) as our does, less-educated people may feel themselves slightly degraded and thus withdraw from political involvement—a motivational rather than a learning result of schooling—which could help explain at least some part of the correlations Jennings reports.

The problem of complicated ideas emerges most clearly in a comparative context. Thus, the Barnes, Kaase study, to which Jennings provided so much of importance, reports lower levels of ideological conceptualization in the U.S. than in the Netherlands, although each country presents a more or less equally difficult ideological continuum (the latter complicated by religion, the former by the absence of a strongly socialist mass party) and despite the fact that the U.S. sample contains over twice as many college-educated as does the Netherlands (19-8 percent) (Barnes, Kaase et al., 1978, pp. 246, 269). In the U.S., apparently, the more difficult the political conceptualization the poorer the return on education becomes, a result entirely consistent with my argument.

What I found disturbing in Jennings' letter is its absence of reference to what I consider the heart of my piece, namely, the last two sections on the basic shape of schooling and on the effects of American democratic pressures on that shape. I wish I could believe Jennings' assertion that our differences on these points are "not so serious," for then I could assume he accepts my argument. I have a feeling, however, that our differences are more serious than he admits, and, at the risk of becoming tedious, I should like to address the problem again briefly, lest the statistical trees Jennings discerns distract us from the forest. After all, the seriatim recitation of correlation coefficients based on indices or even scales of values and information, useful though it is, does not capture very well the texture of social and cultural adaptations to politics that I was trying to describe.

I contended that schooling is basically hierarchical, and I implied that schools function best in teaching complicated democratic ideas when they frankly admit the hierarchical structure of education, and thus are able to legitimize it with their own expertise. I argued that American schools were reluctant to proceed in this way, because they interpreted democratic values as an attack on their own expertise, on rank, on grading, and even on intellectual distinction. I then argued —in contrast to the hidden curriculum theorists— that schools *do* teach democratic values, but more in the form of tacit practices and "styles of behavior" than as complex cognitions.

Let me provide some examples. My 10-year-old daughter informs me that in her class they learn about "rights," such as the right of children not to be struck by other children, the right to share, and the right to take turns. She knows of no other rights. Too early, you say; give the schools time. Well, my 13-year-old tells me that in his classroom they don't talk much about rights in their social studies class; once somebody suggested discussing the rights of homosexuals, and everybody laughed nervously. His textbook lists the Bill of Rights and says that these rights are good things, but it never gets around to explaining why they are good, and why these rights are better than some others—or none at all. His teacher tells me that some of her colleagues do teach democratic theory, but the practice varies from teacher to teacher. She didn't seem too worried one way or the other, but she did worry whether students are "good" citizens in her classroom and whether or not they are nasty to each other in classroom discussions.

My daughter says that her principal gave a little talk a couple of weeks ago about democracy; he said, "We live in a democracy, and we should be graateful for our school and our principal." He wasn't explicit about how these things fitted together. I don't think he is a cynical man or an "authoritarian." True, his sentence is self-serving, but it also reflects puzzlement. He wants to believe in democracy and he wants to retain his authority, but my guess is that he is uneasy about the connection between his two goals. His teachers aren't much up to the theory of the thing either, and it is certainly not a good idea to teach ideas which could turn into ammunition students might fire against the school. If you're not sure you're any better at democratic theory than your students are, maybe it's better to ignore the subject, and instead emphasize "good school citizenship" and informal democratic practices.

Well, these are just anecdotes, unrepresentative statistically and perhaps biased in the interpretation and the telling. But I suspect cultures record themselves as surely in anecdotes as in tables of coefficients, and I think that by now Jennings and I have provided a choice along this line. I only ask the reader to talk to some kids before deciding which of us—if either—is right.

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Comment on Born
(Vol. 73, September 1979, pp. 811-17)

TO THE EDITOR:

Richard Born, in a recent article in this *Review* (1979), purports to refute Erikson's finding (1972) of a relatively uniform increase in incumbency advantage. Born concludes (p. 816) that "No general, pro-incumbent trend has emerged; rather, incumbents in the aggregate have become more secure only because freshmen elected since 1966 have derived greater electoral rewards from their first term." Such a finding, if substantiated, has obvious implications for efforts to find the causes of improved incumbent performance. If we accept Born's results, we should be looking for something which would affect the *timing* of the bestowal of incumbency benefits, not something which would cause a quantum leap in the size of these benefits.

Born's Analysis

Born maintains that Morris Fiorina's recent work (1977) on the relationship between the growth of the bureaucracy and increased incumbency advantage suggests two readily testable hypotheses. First, the first-term incumbents (sophomores) are largely, if not solely, responsible for the recent increase in incumbent performance and second, that this changed performance on the part of first-term incumbents is a result of generational replacement. The second argument is clearly attributable to Fiorina. In an attempt to reconcile the gradual growth of the federal bureaucracy with the much more abrupt shift in incumbency advantage, Fiorina argues (1977, pp. 54-55) that the relatively large turnover in congressional membership in the mid-sixties resulted in an influx of new members more highly attuned to the potential benefits of a home style which emphasizes the role of the congressman as an intermediary between his constituents and the bureaucracy.

The pattern produced by generational replacement should be a sudden improvement in 1964-66 freshman performance. This improvement would be restricted to freshmen in that all veterans would be members of pre-1964 cohorts. In the succeeding election pairs, veteran performance should gradually improve as the veteran ranks continue to be swelled by post-1964 members who are replacing older-generation veterans who have left the House due to retirement or defeat. As Born himself reports, his analysis demonstrates the reverse, with veterans improving their performance suddenly in 1964-66, then declining in subsequent elections. Born nonetheless continues to maintain that his results support Fiorina's generational replacement hypothesis.

What Born has in fact constructed is a view of generational replacement quite distinct from that described by Fiorina. Born's new generation is distinguishable from the old generation only in regard to their improved performance in their first reelection attempts. In subsequent reelection bids, they derive no more advantage from their incumbency than do members of the old generation. Thus, Born's findings are at odds not only with past empirical findings, but also with the theoretical framework in which Born himself casts them.

Born's Measures

In order to test his hypothesis of sophomore culpability, Born analyzes data on congressional election returns for the period of 1952 to 1976. Born measures incumbency advantage in two distinct ways. First, in demonstrating the overall shift in incumbency advantage, he presents the relatively straightforward average incumbent electoral margin, adjusted for national party trends. He finds a mean increase of about 3 percentage points in incumbent safety in the period after 1965 compared to the pre-1965 period. Turning to the question of first-term versus veteran performance, he changes his measure to the average inter-election vote shift, adjusted for party trends. This change is unfortunate. While the former measure taps the phenomenon of interest fairly directly, the latter fails to measure this phenomenon and is biased toward a finding of limited veteran improvement.

Bias is introduced when inter-election vote shift is used as a means of comparing first-term performance with veteran performance. The measure is simply not comparable across these groups. For first-term incumbents, the degree to which they run ahead of their first election bid is a reasonable, and often-used, measure of incumbent advantage, as it is a comparison of an incumbent performance to a non-incumbent performance. The same is not true for veteran incumbents. Here the comparison is between two incumbent performances.

Perhaps the easiest way to illustrate this incomparability is to examine these two measures for a set of hypothetical election data. Table 1 contains the average electoral margins for candidates making their initial election bids (assumed to be 55 percent), their first reelection bids (assumed to be 57 percent), as well as for those incumbent candidates seeking reelection for at least the second time (assumed to be 65 percent). In addition to the average election returns for these groups in ten hypothetical elections, Table 1 contains the inter-election vote shifts that would be generated by the given margins. A uniform turnover rate of 20 percent per election, produced by the departure of veteran incumbents, is assumed.

It is also assumed that no change in the advantage of incumbency has taken place across this period; however, between elections four and five, the members of our legislature enacted legislation giving incumbents a 5-percent bonus to be added to their share of the vote in all future elections. Looking at the pre/post summary of average performance, the effect of this act on incumbent margins is quite clear: both first-term and veteran incumbents average five points higher in the elections following the act than they did in those preceding it. The resulting inter-election vote shifts, however, are not so straightforward and merit an explanation of their computation. The inter-election vote shift for first-term incumbents consists of nothing more than the difference between average first-term performance and average initial election margin. Thus, before the enactment of our five-point incumbent bonus, first-term re-election bids averaged two percentage points higher than initial elections, and with the addition of the five-point bonus, averaged seven points higher.

The computation of inter-election vote shift for veterans is somewhat more complicated. In moving from election one to election two, for example, 80 percent of the veterans in election two were also veterans in election one and thus averaged 62 percent in both elections, for an inter-election vote shift of zero. The remaining 20 percent of the veterans in election two were first-term incumbents in election one, and thus average 57 percent in election one and 62 percent in election two, for an inter-election vote shift of five points. The overall average veteran vote shift is only 1 percent. The movement from election four to election five produces a substantially different inter-election vote shift, due to the addition of the five-point bonus. Eighty percent of the veterans were veterans in both elections and are thus moving from 62 percent in election four to 67 percent in election five due to the bonus. The remaining 20 percent are doubly blessed. Their average first-term performance of 57 percent in election four is boosted five points as a result of their movement into the veteran category *and* an additional five points as a result of the bonus, yielding a total shift of ten points. The result is an overall veteran inter-election vote shift of six percentage points. By election six, however, the effects of the five-point bonus have been absorbed in higher levels of both first-term and veteran performance, with the result being that the remaining inter-election vote shifts reflect only the traditional one-point gain for the veteran category produced by turnover.

If one were to analyze the effects of the five-point bonus using Born's approach of comparing inter-election vote shifts, the implications would be rather startling. A plan which at the outset seemed eminently fair now appears rather severely biased in favor of first-term incumbents. First-term incumbents, as a group, appear to have received full advantage from the legislation (as evidenced by the five-point increase in their inter-election vote shift following the legislation). Veteran incumbents, on the other hand, exhibit an average improvement of only .8 percentage points in their inter-election vote shift in the post-legislation period. Clearly, a comparison of inter-election vote shifts across these two groups is not

Table 1. Hypothetical Election Results (Percent)

	Average Incumbent Vote			Inter-Election Shift	
Election	Initial Election	First-Term	Veteran	First-Term	Veteran
1	55.0	57.0	62.0	+2.0	+1.0
2	55.0	57.0	62.0	+2.0	+1.0
3	55.0	57.0	62.0	+2.0	+1.0
4	55.0	57.0	62.0	+2.0	+1.0
5	55.0	62.0	67.0	+7.0	+6.0
6	55.0	62.0	67.0	+7.0	+1.0
7	55.0	62.0	67.0	+7.0	+1.0
8	55.0	62.0	67.0	+7.0	+1.0
9	55.0	62.0	67.0	+7.0	+1.0
10	55.0	62.0	67.0	+7.0	+1.0
Pre-bonus		57.0	62.0	+2.0	+1.0
Post-bonus		62.0	67.0	+7.0	+1.8
Shift		+5.0	+5.0	+5.0	+ .8

Source: Compiled by the authors.

a very useful way to assess changes in electoral advantage.

Given the enormous simplification of electoral reality in the above example, the similarity of the results to those presented by Born is striking. Born finds a durable increase in inter-election vote shift for sophomores after 1965, but finds only a momentary surge for veterans. As our hypothetical example has made clear, *such a result is by no means incompatible with a uniform increase in incumbency advantage across tenure levels.* The error is not in the inter-election vote shift figures themselves, but lies instead in Born's treatment of sophomore and veteran vote shifts as comparable measures of incumbency advantage. In sum, Born's findings do not provide convincing support for either Fiorina's notions or for Born's own distinctive view of generational replacement.

JOHN R. ALFORD
JOHN R. HIBBING

Oakland University

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Reply

TO THE EDITOR:

John Alford and John Hibbing have misrepresented my findings in a number of important respects. Their central allegation is that I have not substantiated the hypothesis that generational replacement is responsible for the dramatic rise in the safety of House incumbents since the mid-1960s. Specifically, I am accused of the following two transgressions: failing to recognize that the pattern of inter-election shifts presented in my Table 2 (p. 814) could have been produced by a uniform increase in the electoral advantage of incumbency across seniority levels, and controverting myself by reporting a decline in veteran performance after the putatively safer new generation members have been added to their ranks.

The bulk of Alford's and Hibbing's efforts consist of an eminently sensible "simulation" of what we could expect to see if all incumbents regardless of seniority were to receive simultaneously the same one-time electoral boost. The inter-election changes associated with freshmen and veterans, of course, would abruptly jump by like amounts in that election year. Following this uniform surge, we would subsequently find that biennial shifts between the initial non-incumbent year of election and the first reelection bid would continue to reflect the enhanced value of running as an incumbent, while veterans—experiencing the new margin of safety in both years of the election pair—would see their inter-election shifts settle back to the levels typifying the pre-surge period. These results, assert Alford and Hibbing, parallel those turned up in my original study.

Contrary to Alford's and Hibbing's claim, however, my analysis turns up no such pattern. As revealed in my Table 2 (1979, p. 814), the 1966-68 takeoff point for freshman inter-election gains is one which certainly does not extend to veteran members; the freshman shift increases to .072, at least .037 greater than any recorded previously, while that for veterans actually *decreases* by .007. These opposing trends thus show a sudden large electoral boost confined to first-termers alone, rather than any uniform rise for incumbents generally. Additionally, Alford and Hibbing chose to ignore the fact mentioned in my article (p. 814) that there is virtually no correlation between changes in "sophomore surge" values and inter-election shifts for veterans over the 1966-76 period, once again demonstrating that the margins of these two groups are being propelled by different forces. Thus, their "simulated" election results remain exactly that and nothing more.

Alford and Hibbing further misrepresent my article, as well as disregard their own later statements, when they argue that I report an enervation of veteran strength as new-generation freshmen replace departing old-generation members. But as they themselves subsequently recognize, my analysis of veteran inter-election shifts focuses solely on how the performance of incumbents at t_1 *compares* with that at t_2, rather than on the electoral effect of generational replacement. This analysis, then, certainly was never intended to reveal whether the larger first-term vote gains of members elected to Congress since 1966 have been translated into increasing veteran safety. However, Alford and Hibbing neglect to mention that this question is in fact addressed and answered affirmatively in the next section of the article (1979, pp. 814-16),, where it is plainly demonstrated that the replacement effect is substantially stronger during the new generation period than before.

Alford and Hibbing, then, have attacked a set of straw men, rather than the actual contents of the study. Fiorina's hypothesis that a generational transformation in electoral proficiency has produced the greater safety of House incumbents since the mid-1960s is not imperiled by this communication. However, the particular causes of this new-generation safety—whether arising from more intensive cultivation of their districts or elsewhere—still remain to be isolated.

RICHARD BORN

Vassar College

Comment on Ransom (Vol. 74, March 1980, pp. 141-48)

TO THE EDITOR:

In his review essay, "Being Intelligent About Secret Intelligence Agencies," Harry Howe Ransom wrote:

> Until disproved, the following hypotheses, partly intuitive, appear to represent reality: intelligence systems tend to report what they think the political leadership wants to hear. . . (p. 147).

The view that intelligence caters to the preconceptions of the political leadership seems widespread in both academic and journalistic circles. Intelligence practitioners know it to be untrue.

One factor does, unavoidably, exert pressure toward conformity on the individual intelligence analyst. That is the need to defend and argue cogently for whatever position one takes. If an analyst takes a position that is controversial or counter to the conventional wisdom, that analyst will have to do more. He or she will have to do more research, defend the position in greater detail, and the process of review, approval, and coordination will be longer and slower. That is obviously the way it should be; analysts should not be able to rip off an unconventional analysis and send it to the White House without the most careful review. But when analysts are working under extreme time pressure, as most analysts are, it means they have to make a decision whether or not they have the time to do what is required to defend a novel or minority view.

Obviously, this does exert some pressure toward conformity, but several points are important in interpreting the significance of this fact. This is not necessarily conformity to the views of the political leadership. It is, first of all, pressure toward agreement with analysts in competing centers of analysis, e.g., for the CIA analyst, the tacit or active support of colleagues in the State Department and Defense Intelligence Agency who might otherwise seek to undermine one's analysis.

Secondly, it is conformity with the preconceptions of one's own most immediate supervisors. Pressures to reflect one's supervisor's views or to support departmental interests differ in different agencies. They are perhaps strongest in the Defense Department and almost certainly weakest in CIA, where a long tradition of analytical independence supports the individual analyst's freedom to express unconventional or unpopular views as long as they are well documented. Analysis biased in support of long-standing departmental interests is not the same as analysis that tells the political leadership what it wants to hear; students of bureaucratic politics are well aware that departmental interests frequently lead to intelligence assessments that diverge from what the political leadership wants to be told.

Analysts are subject to many countervailing pressures. The need for self-esteem and pride in one's work is, in most cases, at least as strong as pressures for conformity. In 28 years' service with CIA, I found that analysts generally did not hesitate to advance a novel or minority view even though it might make a few waves or cause them more work. Disagreement is, in fact, endemic to the intelligence community, and is far more prevalent than conformity. Most analysts enjoy challenging the superficial or conventional wisdom of the policy makers or other analysts, and they look for opportunities to do so as this is how they can be most useful. Analysts gain the greatest job satisfaction from presenting and defending their own views under precisely those circumstances when, according to Ransom's hypothesis, they should be catering to the preconceptions of the political leadership.

RICHARDS J. HEUER, JR.

Naval Postgraduate School

Reply

TO THE EDITOR:

Most generalized observations of political behavior fail to encompass the complexity of reality. The generalization to which Richards J. Heuer objects comes from my diligent reading of nonsecret sources over the past quarter-century. From this evidence and from conversations with intelligence professionals, I intuitively suggest the hypothesis that intelligence agencies, even if unconsciously, tend to report what they think decision makers want to hear. Heuer's experience is different from mine and he reaches a contrary conclusion. He has had the advantage, and disadvantage, of the inside view of a secret agency. I respect his observations.

Yet how does one explain the monumental intelligence failures beginning with Pearl Harbor? How does one interpret the shock in Washington at the first Soviet atomic bomb explosion in 1949; the dual surprises in the Korean War—the initial attack and later Chinese intervention; the Bay of Pigs failure; the Cuban missile surprise; the colossal failures in Vietnam, early and late; the frequent failures in forecasting Soviet strategic military power; and the more recent intelligence failures regarding Iran and Afghanistan? Surely an information pathology is at work. Admittedly, the concept of "intelligence failure" presumes a theory of intelligence that remains underdeveloped.

Failure can occur at any number of points in a complex series of steps, from the setting of requirements to the use of intelligence by decision makers. But in the examples cited, the decision makers in each instance were misinformed and in most cases were pleased to hear the intelligence estimates given them.

The inference I draw from these and other examples is that tendencies exist in bureaucratic structures that lead to misinformation or worse. I agree with Mr. Heuer that such tendencies may be less pronounced in the CIA than in department intelligence reports. Certainly such an objectivity was an important reason for creating the CIA in 1947. The practice in recent times of appointing CIA directors anew, with each new incoming presidency, however, invites a dangerous politicization of central intelligence, exacerbating the problem of objective reporting.

The record clearly indicates that major intelligence estimates have been tragically wrong about many major events over the past 30 years. Doubtless numerous variables are involved in these failures. Pressures on, and tendencies of, intelligence professionals to bring good news to political leaders appear to have been important causes of such failures.

HARRY HOWE RANSOM

Vanderbilt University

Reply to Schrodt
(Vol. 74, December 1980, p. 1048)

TO THE EDITOR:

Philip Schrodt has two complaints: one hits the target and the other misses the mark by a wide margin.

The target-hitting complaint is, of course, that one cannot derive

$$\frac{dx}{dt} = ky - ax + g \text{ from } \frac{d^2x}{dt^2} = -a.$$

My contrary claim was, I grant, worse than a fault; it was a mistake and one which I shall now put right. Replace

$$\frac{d^2x}{dt^2} = -a \text{ with } \frac{d^2x}{dt^2} = k\frac{dy}{dt} - a\frac{dx}{dt} \text{ from which}$$

$$\frac{dx}{dt} = ky - ax + g$$

can be derived using, *pace* Mr. Schrodt, freshman calculus. Even Schrodt's target-hitting arrow, however, falls short of a bull's-eye since the *whole point* of the discussion of that section (pp. 108-09) was to argue that political scientists who think of "explanation" in international politics as conforming to a highly idealized hypothetico-deductive model misconceive both the nature of explanation—which lies outside positivist science—and international studies—which lies outside positivist conceptions of it. Given the validity of the corrected notation and Schrodt's silence about my general point, I shall assume that my argument holds good. Indeed, the notion that a "theory" of international politics ought to conform to some single criterion of rationality is, on my view, a self-protective piece of petty legislation which precludes the possibility of counterexamples by stipulating that there are none. This point is not new but in light of the undeserved popularity of the H-D Model among some political scientists, it bears repeating.

As to Schrodt's second point concerning my having attributed to Richardson a deterministic *Weltanschauung*, the arrow is crooked. I never said that Richardson was a determinist. What I said was that he had committed himself, whether he realized it or not, to a version of scientific determinism, and this still seems right to me. From the fact that Richardson "sold his physics books" to learn about statistical proof from Karl Pearson it does not follow that he was not committed to a version of scientific determinism. As determinists have long argued, mere indeterminacy or randomness goes no way toward providing the possibility of free will. To have free will requires that an agent could have acted otherwise.

Let me briefly reconstruct my argument at pp. 112-13 of the article in a more cryptic form. Suppose, as the quotation from Richardson suggests, there are only two alternatives for nations—namely, *either* they follow fixed traditions mechanically *or* they stop and think. Then we have to choose between the second conjunct whose truth-conditions can never, so far as we can ever tell, be decided upon (leaving to one side the far-fetched), and the first conjunct. This is no choice at all except in a rhetorical way and that leaves the first conjunct. But what does it mean to say that nations follow fixed traditions mechanically, etc.? It appears to mean that laws of human behavior specifying necessary and sufficient conditions would suffice to determine the choices of political leaders such that they could not but act as they do. It is hard to see how Richardson could escape, *malgré lui,* a commitment to scientific determinism. Of course, I am aware that in the *Statistics of Deadly Quarrels* Richardson "discovered" that the typical pattern of such conflicts turned out to be distressingly random, but it does not follow that he did not commit himself to the search for laws governing human behavior. Support for this claim may be found in Rapoport's observation (1968, p. 516) that Richardson "hoped" that "the [sic] laws governing human violence would emerge," i.e., that sufficient antecedent conditions could be found which would give the causes of human behavior. Such laws would present a sufficient cause for an effect; the effect could not but follow; that that, in my terms, just is scientific determinism.

Despite Richardson's discovery of the purely random relationship between outbreaks of war and returns to peace, many of his followers have continued the search for the laws of human violence without even raising the question of whether Richardson's commitment to determinism is coherent. In my view it is not, but I cannot go into that here. In my article I thought it sufficient to smoke out this issue from the mathematical notation in which it has long been embedded and to call the bluff of those mathematical imperialists who think that adding a few more variables will yield determinant laws of human behavior. In general, whether I succeeded or not is unimportant, for the key point is to uncover the many and varied ways in which political scientists repeat the mistakes of philosophers and thinkers they refuse to read.

Why bother with these errant eristics? One reason for it has to do with what I take to be the intimate tie between the possibility of an essentially Cartesian conception of knowledge, glossed as logic and mathematics, and the "sad state" of moral, legal and political theory in the contemporary study of international politics. Once we see that the claims advanced on behalf of the former are quite unjustified—or, at best, as Hegel put it in arguing against Kant's Categories, one-sided, empty formalism—we shall, I hope, be better disposed to a radical shift toward the virtually uncharted region of substantive moral and political theory in international studies.

Let us, then, drop this cant about misinterpreting Richardson, Marx, Spegele, and Schrodt, and recognize the truth in Mathew Arnold's calculation that fully three-fourths of human life is occupied by moral concerns, and that international studies can and ought to carve out a significant role for itself in explaining our often tortuous moral decisions about peace and the avoidance of the horrors of war:

> Nor milder is the general lot
> Because our spirits have forgot
> In action's dizzying eddy whirled
> The something that infects the world.
>
> —Matthew Arnold, "Resignation"

ROGER D. SPEGELE

University of Nairobi

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ON BOOK REVIEWS

Comment on Campbell's Review (Vol. 74, September 1980, pp. 835-36)

TO THE EDITOR:

John C. Campbell's recent review of my *Suburban Tokyo* contains some errors, distortions, and misrepresentations that warrant the attention of *APSR* readers.

His most egregious error appears when he states that "these data are aggregated only at the city level" (p. 836). This statement is simply false, because tables in the book present data for many different levels in addition to the city level. The review implies that sub-city data do not appear owing to an oversight on the author's part. In fact, such results do not appear because they are not available: Japan's election law precludes tabulation of voting returns at the precinct level.

One distortion arises because Campbell believes that I try to account for political differences between two suburbs with a "hypothesis" concerning divergent migration patterns. Unfortunately, arguments in the book are merely generalizations based on limited evidence, not hypotheses reached through deductive analysis. Moreover, although I do stress the importance of migration patterns, I also analyze at least five other causes for the differences between the two cities, stemming from urban development, mayoral constituencies, party organization, union behavior, and neighborhood associations. Since Campbell reveals an understanding of only one of these six explanations, readers will readily appreciate why he did not find my arguments "convincing."

Misrepresentations occur owing to a mistaken understanding of the mode of discourse in history. Campbell states that I claim to write "as historian, anthropologist, sociologist, and political scientist" (p. 835). The book states that I "selectively appropriated the concepts, techniques, and methods of several disciplines" but that I write "as an historian" (p. xi). This is a significant distinction, because writing as an historian (by contrast with thinking like a physicist) imposes a style that is both analytic and ideographic. The subtleties of discourse in history are conceived to represent faithfully the "amorphous characteristics" over which Campbell despairs. In their perversity, historians think that reality itself is amorphous, or, to put it another way, we feel that man does not live by theory alone.

GARY D. ALLINSON

University of Pittsburgh

Comment on Fowler's Review (Vol. 74, December 1980, p. 1092)

TO THE EDITOR:

Linda Fowler's generally accurate review of *Parties, Interest Groups and Campaign Finance Laws,* which I edited for the American Enterprise Institute, said that the "proceedings are dominated by practitioners of various political backgrounds and ideological views," without naming the participants. I thought potential readers might want to know that although the book included some papers by practitioners, three of the four panels were based on papers by political scientists —Edwin M. Epstein, Ruth Jones, Xandra Kayden, Herbert Alexander, and Khayyam Z. Paltiel, as well as myself. The comments on the papers did come mostly from practitioners, but commentators David Adamany and George Agree are well known for their writings on the subject.

MICHAEL J. MALBIN

American Enterprise Institute

EDITORIAL NOTE

A second research frontier article appears in this issue. Dorothy Nelkin, of Cornell University, identifies dimensions of the accident at Three Mile Island that are proper subjects of social science research. David Sills of the Social Science Research Council was asked to comment on her article. A third article will appear in the September 1981 issue. The topic is the political representation of women, as analyzed by Virginia Sapiro, University of Wisconsin.

The following articles have tentatively been scheduled to appear in the June, 1981, issue:

Harvey C. Mansfield, Jr., Harvard University, "Machiavelli's Political Science"

Robert Axelrod, University of Michigan, "The Evolution of Cooperation among Egoists"

Trudi C. Miller, National Science Foundation, "Political and Mathematical Perspectives on Educational Equity"

Robert M. Stein, Rice University, "The Allocation of Federal Aid Monies: The Synthesis of Demand- and Supply-Side Explanations"

Richard L. Engstrom and Michael D. McDonald, University of New Orleans, "The Election of Blacks to City Councils' Clarifying the Impact of Electoral Arrangements on the Seats/Population Relationship"

C. Neal Tate, North Texas State University, "Personal Attribute Models of the Voting Behavior of U.S. Supreme Court Justices: Liberalism in Civil Liberties and Economics Decisions, 1946-1978"

Robert H. Dorff, North Carolina State University, and Jürg Steiner, University of North Carolina-Chapel Hill, "Political Decision Making in Face-to-Face Groups: Theory, Methods, and an Empirical Application to Switzerland"

Robert H. Salisbury and Kenneth A. Shepsle, Washington University, "Congressional Staff Turnover and the Ties-that-Bind"

Barbara Sinclair, University of California-Riverside, "The Speaker's Task Force in the Post-Reform House of Representatives"

Joseph Cooper, Rice University, and David Brady, University of Houston, "Institutional Context and Leadership Style: The House from Cannon to Rayburn"

Donald S. Collat, Yale Law School, Stanley Kelley, Jr., Princeton University, and Ronald Rogowski, Duke University and University of California-Los Angeles, "The End Game in Presidential Nominations"

James H. Kuklinski and Darrell M. West, Indiana University, "Economic Expectations and

Voting Behavior in United States House and Senate Elections"

D. Roderick Kiewiet, California Institute of Technology, "Policy-Oriented Voting in Response to Economic Issues"

Errata

The figure published in Roy Pierce's review (September 1980, pp. 871-72) of *Conflict and Consensus in France*, edited by Vincent Wright, was given incorrect source information. The source of the data on which the figure is based is *Le Nouvel Observateur*, 24 April 1978, cited in Wright's book on p. 39. We apologize for this error.

* * *

Because of an unfortunate layout error, an incorrect footnote appears in Yoav Peled's article, "Rousseau's Inhibited Radicalism: An Analysis of His Political Thought in Light of His Economic Ideas" (December, 1980). On p. 1040, footnote 5 should read as follows:

[5] A growing population is, for Rousseau, the only sure sign, and an inevitable result, of good government. *Poland*, p. 230; *Corsica*, p. 83; *Emile*, p. 432.

BOOK REVIEWS

Political Theory and Methodology

Gramsci and the State. By Christine Buci-Glucksmann; trans. by David Fernbach. (Atlantic Highlands, N.J.: Humanities Press, 1980. Pp. xiv + 470. $35.00.)

Along with Lukács and Korsch, Antonio Gramsci is considered by many a founder of Hegelian Marxism. By perceiving history as a living totality that unfolds dialectically, and modern capitalism as a momentary battleground for opposing economic classes, Hegelian Marxists emphasize human cognition and action. Workers, punished unmercifully by capitalism's exploitative conditions, will subjectively experience the revolutionary role history has allotted them, seize the productive apparatus, and create socialist justice. While the immediate cause is a worsening of the material contradiction between means and relationships of production, this entire episode is merely one fragment of an abstract universal process. Hence revolutionary action expresses the confluence of objective material factors and a subjective awareness of what it all means. The former supplies propitious revolutionary conditions; the latter initiates action. Marxists must therefore cultivate an apposite working-class consciousness, rejecting orthodox economism and recognizing subjectivity as an irreducible historical reality.

Hegelian Marxism attracts Westerners disillusioned with bourgeois rhetoric but also dismayed by the mechanical anti-subjectivism of materialist Marxism. Extraordinarily intelligent and principled, Gramsci is a revolutionary who also uninhibitedly condemns the Second International and lauds "progressive" liberal freedoms. Predictably, his popularity in the West is growing. Christine Buci-Glucksmann's *Gramsci and the State* tries to extract Gramsci's Marxist reputation from this suffocating embrace. The principal hypothesis fits Gramsci squarely in the orthodox materialist tradition. Apparent disparities between his emphasis on willful action and orthodoxy's passive materialism are resolved in Gramsci's expanded notion of the state, which simultaneously represents reflective action (praxis) and history's objective material laws (theory).

Buci-Glucksmann convincingly documents the evolution of Gramsci's "integral state" from its origins in 1918-20, to its mature exposition, after 1926, in the *Prison Notebooks*. For orthodox Marxism, the state is a political apparatus by which one class coerces another, a cultural tool of oppression automatically ceded to the dominant economic class. Gramsci's state is more dynamic. Still a political conduit for class rule, it is also the means for constituting civil society as an organic unity embracing all segments of the populace. The state's power, therefore, comes from its coercive apparatus as well as the entire complex of theoretical and practical activities used by a ruling class to rationalize its dominance and win the masses' active consent. With these "hegemonic" apparatuses—books, journals, schools, pulpits, air waves, museums, etc.—a state creates an "historic bloc," an organic unity between leaders and followers. Intellectuals perform the vital function of legitimizing culture, including extant class relationships. Hence, a proletarian "war of position" is needed, where working-class intellectuals create the subjective conditions required for the revolutionary transformation of one historical bloc to another. In brief, the emerging proletarian state reflects objective class antagonisms *and* actively, cognitively smashes elite hegemony.

This makes perfect sense from a Hegelian Marxist point of view, emphasizing the irreducible significance of the superstructure. Buci-Glucksmann, however, argues that Gramsci is a materialist. The integral state, as revolutionary praxis, dialectically expresses an objective material reality which in the last, non-immediate instance is determining. Subjectivity is saved, but not at the expense of materialism.

Buci-Glucksmann assiduously traces Gramsci's lifelong admiration for Lenin's activist spirit and revolutionary tactics. However, the controversial hypotheses—Lenin's saving materialism from an economistic death, and Gramsci's adoption of Lenin's subjectively inspired materialism—are undeveloped. The first is based on Lenin's rambling *Philosophical Notebooks*, where he inconclusively seeks to justify political activism theoretically. Buci-Glucksmann conveniently ignores Lenin's single published work on epistemology, *Materialism and Empirio-Criticism*, which is rigorously orthodox. She also quietly assumes that Lenin's revised materialism is philosophically compelling —which is problematic. Lenin's *Notebooks* may well have served not to alter materialism substantively, but to guarantee a party activist the freedom to do whatever he or she wants without violating history's objective material laws. In this case, the connection between Lenin and Stalin is, notwithstanding the author's disclaimers, irrefutable. The second issue, Gramsci's epistemological debt to Lenin, is also unproven. Although both were flexible party activists, the long-run philosophical implications of Lenin's materialism—i.e., contemporary Marxism-Leninism—vitiate Gramsci's concern for reflective praxis. Since, in addition, Gramsci openly criticized *Materialism and Empirio-Criticism,* never even read the *Philosophical Notebooks,* traced his own philosophical heritage in Hegel and Croce, and continually spiced his commentary with Hegelianisms, it is unrealistic to presume Gramsci was, philosophically, a Leninist.

Buci-Glucksmann avoids these unresolved theoretical problems by arguing that Marxism is anti-philosophical. This thesis arrogantly ignores the enormous body of significant Marxist literature devoted to facilitating rebellion by explicating Marxist philosophy, i.e., explaining to intelligent people why revolution is "correct," capitalism "oppressive," and socialism "liberating"—a task Marx himself left unfinished. It is also queer to read an Althusserian structuralist who denies the relevance of philosophy in order to save "authentic" Leninist materialism.

Gramsci and the State should be read very carefully. It lucidly outlines Gramsci's innovative theory of the state. It also dogmatically squeezes Gramsci into the author's own self-serving theoretical niche. Ironically, it mirrors the best and worst of bourgeois social science.

ROBERT A. GORMAN

University of Tennessee

Merleau-Ponty and Marxism: From Terror to Reform. By Barry Cooper. (Buffalo, N.Y.: University of Toronto Press, 1979. Pp. xv + 223. $17.50.)

History and Human Existence: From Marx to Merleau-Ponty. By James Miller. (Berkeley: University of California Press, 1979. Pp. xi + 287. $17.50.)

The failure of Marxism as a mode of historical explanation has not detracted from its status as an existential mode. It continues to motivate and justify the principle of revolt against bourgeois life. Sartre, Malraux, and perhaps most interestingly, Merleau-Ponty, continued to share the moral imperative to revolt after they had rejected the empirical necessity of revolution. James Miller has written a book arguing that men like Merleau-Ponty are the proper heirs of the Marxist spirit, while Barry Cooper has written one both explaining Merleau-Ponty and showing the extreme tension within his thought.

James Miller writes in his introduction:

> My real reason for taking up the question of subjectivity with regard to Marxism (rather than some other social theory) lies in the irrevocable commitment of revolutionary Marxism to abolish all social orders in which human beings are oppressed and degraded. That commitment seems to me the proper place for contemporary social thought to begin—even if it ends by discarding Marxian theory (p. 9).

If one wishes to abolish oppression, then there are two reasons to abandon Marx. First, Marx seems to have based his argument for the abolition of oppression on historical necessity rather than on moral sentiment. History has thus far failed to abolish suffering and shows little inclination to do so autonomously. One might wish to compel history to do one's bidding, but one first needs a theory of agency other than that supplied by Marx. Second, one must abandon Marx because those regimes created in his name have been at least as oppressive and degrading as those which they claim to stand against.

It is thus necessary to develop a theory which is revolutionary in intent and which leaves room for voluntary action in history and concern for the individual as history's end. James Miller's book is an attempt to discover such a concern embedded in Marx, and to show how this concern culminates in contemporary varieties of existential Marxism, and in particular, in the writings of Merleau-Ponty.

Miller develops Marx's concern for autonomy by introducing the notion of "individuation." Miller distinguishes individuation from individualism. He means by individuation, "the process whereby human beings become distinctive, autonomous and self-conscious agents, each capable of purposefully reshaping the natural world and of independently evaluating moral claims" (p. 5). In a way, this is appropriate, inasmuch as Miller's reworking of Marxism seems to make intellectuals the new revolutionary subject.

Now Miller is certainly correct in seeing the creation of human autonomy as an end of Marx's project. However, his notion that the independent evaluation of moral claims is the essence of autonomy for Marx is more problematic. Marx is not Kant. First, moral evaluation is never individual. It is historical and social: morality is object, unfree. In the trans-historicity of communism, morality is abolished as is the ruthless criticism of everything existing. Both historical heteronomy and the trans-historical abolition of the moral problematic undermine Miller's argument about the nature of Marx's individuation.

Miller obviously knows this. He therefore argues that this theme of Marxism finds its fullest expression in the sort of existential Marxism of which Merleau-Ponty was perhaps the finest specimen (p. 198). In his political thought, Merleau-Ponty was archtypical of the contemporary radical intellectual. Committed to the redemption of mankind, his life was a series of disappointments about various movements of the left, without losing hope that something would turn up.

Marx's historical prophecy had proved faulty and the regimes created in his name proved inhuman. One might abandon the principle of the revolutionary redemption of mankind, but then the intellectual would be nothing more than the purveyor of useful gadgets. Existentialism from this standpoint meant an existential commitment to redemption and a revulsion at bourgeois life. Miller is indeed correct in arguing that Marxism culminates in the sort of thing Merleau-Ponty and Sartre represented, that it is necessary to develop a contingent argument for revolution in our time. But this culmination masks a failure. It undermines the scientific basis of Marxism. This changes Marxism from a universal necessity to merely one value judgment among many in a world where all values are relative. Furthermore, philosophy, which Marx sought to make identical with the proletariat—that is, with humanity—becomes once more the specialized purview of the intellectual, in the hothouse world of the Paris cafe.

Barry Cooper tries to deal with this problem by focusing on Merleau-Ponty alone. As Cooper points out, Merleau-Ponty's Marxism was a peculiarly disjointed affair. Merleau-Ponty, seeing the contingency of history, had rejected Marx's determinism. At the same time, he refused to abandon Marx altogether. Merleau-Ponty possessed what Cooper calls "a mood of hope" (p. 55). Merleau-Ponty faced the opposition between contingency and hope by trying to escape the political reality of the postwar world: the cold war between the hopeless United States and the hope-filled but brutal Soviet Union. Merleau-Ponty sought a middle course between bankrupt liberalism and brutal communism. Cooper asserts that the New Left and Eurocommunism represent that middle way (p. 168).

Both Cooper and Miller seem in sympathy with Merleau-Ponty's radicalism. Cooper, however, seems more sensitive to the troubling logic of Merleau-Ponty's formulations. Marx had refuted liberalism; at the same time, the practical consequences of Marx's thought were unacceptable. Merleau-Ponty's solution was a new political practice, committed in principle to Marx's project. However, this solution was excessively modest, if Eurocommunism is an example. Another possibility, more radical, eluded him—that the practical problem was really a philosophic one: to formulate an escape from the dialectic of modernity itself. Modernity, like Miller, is committed to abolishing all unfreedom. Each attempt has culminated in catastrophe. Modern radicalism searches for technical mistakes in its blueprints, refusing to suspect the project itself. Perhaps a truly radical suspicion ought to be raised: that domination, the rule of some over others, rather than being the quintessence of the inhumane, is the essence of humanity—that ruling and being ruled is what being human is all about.

GEORGE FRIEDMAN

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Perché la violenza?: Una interpretazione filosofica. By Sergio Cotta. (L'Aquila: L.U. Japadre Editore, 1978. Pp. 155. L. 3,500, paper.)

"It is not in the power of an individual to escape from an epoch of violence, much less to put an end to it. But to attempt to overcome it is a fundamental personal responsibility for anyone who has respect for man and thus for himself." So begins the magisterial study of violence by Sergio Cotta, professor of philosophy of law at Rome University and Italy's leading political theorist.

Many scholars write about the practical relevance of political theory, but few have done as much to illustrate that relevance than has Sergio Cotta. In numerous books and articles, he brings the insights of the classics of political theory alive in such a way that they contribute directly to understanding present-day practical problems.

A few years ago, in his book ironically entitled *L'uomo tolemaico,* Cotta advanced the thesis that it is *modern* culture which is "earth-centered" compared to medieval culture which was centered on transcendent reality. Cotta's study should be read in conjunction with Eric Voegelin's *From Enlightenment to Revolution* (Duke, 1975) and Hans Sedlmayr's *Art in Crisis: The Lost Center* (Regnery, 1958), for detailed documentation concerning the enormity of modernity's break with the past.

Much of our political and social science today demonstrates ignorance of this spiritual revolution and of its consequences. As a result, broad sectors of contemporary political science accept so narrow a paradigm of "politics" that even the phenomenon of twentieth-century totalitarianism has been incomprehensible to them. Those who follow the comfortable political science of the present day lack the analytical tools and historical knowledge even to begin to understand a political phenomenon as central to our time as terrorism. Cotta's books should be read by all political scientists so that with his help we all might begin to remedy our ignorance.

In part, Cotta's book might be described as a phenomenology of violence. He takes issue both with those writers who have defined violence too broadly and those who have defined it too narrowly. The difficulty with the first group is that if all politics and all history are seen in terms of violence (*tutto è la violenza*), then the very distinction between violent and nonviolent action becomes meaningless. What Cotta calls the "apocalyptic" theory of violence holds that all institutions of modern "repressive" society are manifestations of violence, for they "violate" the humanity of man by manipulating, indoctrinating, and regimenting him, even if the means used are "peaceful." (Ironically, the enlightenment faith that education would overcome the "violent" proclivities of "primitive" man is itself ridiculed by the view that modern education itself is a form of "violence" against the person.) For Cotta, the radical position is absurd, for it abolishes every distinction between public force and private violence as well as the distinction between violence and nonviolence. Thus, in the radical revolutionary literature even "nonviolence" is seen as a deliberate tactic for provoking violence on the part of the established authorities.

While the "gurus" (Cotta's term) of contemporary radical revolutionary thought are mostly nonviolent, peaceful people themselves, the connection between their doctrines and the actions of the Red Brigades and the Baader-Meinhof gang is a direct one. Indeed, Cotta thinks that the current widespread exaltation of violence and its equation with "morality" is an unprecedented event in the history of thought. Hitherto, even revolutionaries such as Marx, Lenin, and Mao have endorsed violence only as a grim necessity to achieve the victory of the revolution ("political power stems from the barrel of a gun"). They have viewed it coldly and calculatingly as a *means* to achieve an *end*. The new "revolutionaries," on the other hand, glorify violence as itself the end. The violent revolution is to be "permanent."

While exposing the dream-constructions" of the radical revolutionaries to the cold light of reality, Cotta refuses to fall over into another danger and define violence too narrowly as exclusively the work of "private," unauthorized groups. The distinction between legitimate "force" (by constituted public authority) and illegitimate "violence" (issuing from private groups) obscures the truth that even constitutionally established public authorities can fall into patterns of activity that violate the dignity of the person. Ritualistic cries of "law and order" may themselves contribute to a climate of violence.

What is violence, then, according to Cotta? Using the terminology of personalist philosophy, he concludes that violence is "an unregulated, nondialogic, and non-coexistential counter-activity." It is noteworthy that violence can be described only in terms of negations, for it occurs in the sphere of "opaqueness, refusal to care for the other, and arbitrary and absolute constriction."

Today's apostles of violence view identifiable individuals with contempt. The glorification of violence represents the ultimate manifestation of closure and reification. By insisting that violence be characterized only in terms of negations, many political and social scientists may be expected to attack Cotta for having abandoned the "fact-value dichotomy" and the scholar's commitment to "ethical neutrality." Cotta himself seems to be too busy following the trail of political reality to notice that he violated the taboos of today's positivistic social science, however.

Sergio Cotta brings this masterfully written volume to a conclusion in the last two chapters by considering the question posed in the title: "Why is there violence?" Here he does not mean to ask why is there violence as such, but rather why is there the "exaltation" of violence in so much of the contemporary world. It is this exaltation of violence that constitutes what is truly novel in the contemporary situation.

There is no way in a brief review to do justice to the complexity and brilliance of these final two chapters. Cotta ranges over such topics as the influence of Rousseau's voluntarism on democratic doctrine, the advent of industrial, technological society, the "activist historicism" of Marx, the overthrow of what he calls the "juridical measure" by a legal philosophy that reduces law to the expression of a particular will, and "absolute subjectivism" in twentieth-century philosphy. Taken individually, these topics are familiar to political theorists, but Cotta's philosophical framework and flair for nuance bring a new dimension and a refreshing lucidity to their discussion.

Perché la violenza? is one of the major works of political theory published in the last decade. It would be nothing short of a scandal if it is not immediately translated into English and widely discussed here in the United States.

DANTE GERMINO

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The Correspondence of John Locke: Vol. 5, Letters 1702-2198. Edited by E. S. de Beer. (New York: Oxford University Press, 1980. Pp. vii + 800. $89.00.)

Each successive volume of Locke's correspondence reveals fresh vistas on the activities and interests of this extraordinary genius. The nearly 500 letters reproduced in this volume were written or received between January 1694 and February 1697, a period of just over three years. It is awesome to realize that this was but a portion of Locke's total correspondence during those years, especially when one observes that some of these letters are the equivalent of short essays on complex questions of theology, philosophy and delicate affairs of state.

By 1694 Locke was settled in relative security in England; the most perilous years of political turmoil and exile abroad were behind him. His major works had either been published or were nearing publication. His fame was international and his reputation for prudence was such that he was frequently summoned to public service by King William, Lord Somers, and others in high position. Locke had written in *Some Thoughts Concerning Education* that it is "every Man's indispensable Duty, to do all the Service he can to his Country. . . ," and though his health was dangerously frail, he accepted a series of responsible and demanding posts. This portion of the *Correspondence* provides marvelously rich and detailed information on Locke's many contributions to the solution of such pressing problems as trade, colonial and foreign policy, and, above all, reform of England's debased coinage. As Locke wrote to his friend William Molyneux in 1696, "The business of our money has so near brought us to ruin, that . . . it was every body's uneasiness" (p. 594). Deeds followed words, as Locke spurred his closest friends in Parliament, Edward Clarke, Sir Walter Younge, and John Freke, members of his "College" or parliamentary political club, to secure passage of sound legislation leading to currency reform. Parliament acted favorably, and Locke wrote to Clarke assuring him that "I think the nation extremely obliged to you. . ." (p. 509). In the same letter he urged the college to initiate action on a broad front to instruct and marshal public opinion in order to insure the effectiveness of this vital legislation. Locke's letters in this regard are most instructive in revealing the spirit with which leading citizens in the social-contract society must be infused if the prosperity and security of the nation is to be preserved.

Even more fascinating in this portion of Locke's correspondence are the hitherto inadequately discussed accounts of his persistent efforts to conceal his authorship of *The Reasonableness of Christianity*. He steadfastly refused to acknowledge authorship of this controversial work to his closest and most trusted friends, even as he sought their opinion of it! His judicious friend William Molyneux never did "presume to enquire" whether Locke was the author, but he did report that "a very Learned and Ingenious Prelate said to me . . . [when] I asked him whether he had read that Book, and how he liked it; He told me very well; and that if My Friend Mr. Locke writ it, 'twas the best book he ever laboured at; But, says he, if I should be known to think so, I should have my Lawns torn from my shoulders. . ." (p. 702). These and many similar passages from this portion of Locke's correspondence may prove instructive to those who, for whatever reason, continue to experience insuperable difficulties in recognizing the issue of persecution and the art of writing.

ROBERT H. HORWITZ

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Violence and Oppression. By James C. Dick. (Athens: University of Georgia Press, 1979. Pp. x + 211. $12.00.)

Can it be moral for the oppressed to use deadly violence against their oppressors? To answer, James Dick analyzes four such episodes in American history. He concludes that in none of them—the battle of Alamance in 1771, Nat Turner's revolt in 1831, the Homestead labor violence in 1892, and the Colorado Coal War of 1914—was the violence morally justified, and, after further analysis, he concludes that such violence can seldom be justified. Finally, he gives us five criteria that must be met to justify such violence. These are the presence of serious injustice, the failure of less objectionable methods of redress, and avoiding violence that is gratuitous, directed against the innocent, or doomed to failure. Since most violence will not meet all these tests, Dick concludes that deadly violence by the oppressed is "not easily justified even under tyranny" (p. 187).

Does the analysis justify the conclusion? Probably not. The choice of cases, given Dick's assumptions, is peculiar, their interpretation questionable, the generalization drawn from them too broad, the range of possible moral justifications treated too narrow, and the analytical technique used to validate his five criteria too dubious.

James Dick uses case studies because a theoretical analysis not "steeped in the actualities of history" can only mislead (p. x). Yet his argument that to be justified violence must succeed makes his choice of cases most curious. First, he restricts his cases to those of small-scale violence involving at most a few dozen deaths, though it is quite plausible that effective violence is large-scale violence. Second, and still more curiously, he restricts himself to cases in which, by his accounting, violence failed, though he admits that it does not always fail, and in fact cites cases in which violence succeeds. How, then, can these studies of small-scale futile violence support a conclusion applying to violence in general, including large-scale successful violence, especially as success is made a moral test? Why, in view of his assumptions, did Dick not choose a more representative range of cases?

Nor is it clear that Dick interprets his cases correctly. He sees no successes, yet in the Colorado Coal War the power of a corporation that was virtually a state within a state was greatly reduced and, by Dick's accounting, a "brutal despotism" became a more or less "benevolent paternalism" (pp. 64-65). Dick asserts, though not convincingly, that the change was due to public reaction to company violence, not labor violence. But the one was both preceded and followed by the other. How does Dick know which link in the chain of violence was crucial, and how can he divorce one link from the others? Neither is it clear that a series of apparent failures, such as the Homestead violence, may not have a favorable cumulative impact on public policy.

Dick's moral analysis is too narrow. He does not give serious consideration to justifications of violence based on honor or just retribution, he does not seriously consider the possibility that tyrants may abrogate their rights, and he does not treat seriously the possibility that human dignity may in some circumstances require violence even if it fails to end oppression. (Dick dismisses honor with a quotation about dueling; retribution he tends to lump with revenge, which is dismissed; the death of a tyrant is treated as, at best, a lesser evil; and only in the case of the Warsaw uprising does he seem to accept doomed violence, though his explanation would seem to apply broadly.)

Finally, Dick tries to validate his five criteria by using John Rawls' original position analysis. But, just as Rawls' self-interested contractors, ignorant of their desert or lack of it, ignore desert as a standard of distribution, so Dick's contractors, ignorant of their status as oppressors or oppressed, ignore certain questions of honor and justice. Serious questions are not answered in this way.

WILLIAM R. MARTY

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Elitism. By G. Lowell Field and John Higley. (Boston: Routledge & Kegan Paul, 1980. Pp. xi + 135. $18.50.)

In this slim but expensive volume, G. Lowell Field and John Higley advance an unabashedly elitist position, discuss its implications and consequences, and defend it against rival postures.

The "eclipse of elitism" over the last 200 years, Field and Higley argue, is owing to the fact that, beguiled and deluded by socialists or liberal (welfare-state) principles, Western social scientists have erroneously (1) advocated generally egalitarian positions, and (2) assumed that science and technology are capable of addressing problems of resource scarcity and of assuring unlimited growth.

In setting forth their own position, Field and Higley define elites as "persons who occupy strategic positions in public and private bureaucratic organizations" (p. 20) and to whose rule "there is no alternative" (p. 70). The authors then restate "the elitist paradigm" developed in the seminal work of Vilfredo Pareto, Gaetano Mosca, and Roberto Michels by "markedly reducing [its] determinacy" (p. 19).

Thus, whereas Pareto, Mosca, and Michels had stressed the essential autonomy of elites, Field and Higley emphasize *non*-elite orientations and attitudes as serious constraints on elite action. Moreover, the authors hold, non-elite orientations toward societal issues vary with changes in the composition of labor force components as societies go through four levels of development: agricultural, lightly industrial, highly industrial, post-industrial. The direction of this change is from "prevailingly egalitarian" to "prevailingly managerial" (pp. 21-31). Although non-elite orientations influence the types of societal issues that will or will not be raised, given their "very general" nature, elites continue to have much discretion in shaping society (p. 19 and passim).

Similarly, whereas Pareto et al. had stressed the internal homogeneity and unity of elites, Field and Higley emphasize their heterogeneity and diversity. They identify four elite types, arguing that each type determines the intensity and consequences of political conflict in societies at the same developmental level, or at different levels. *Disunified elites*—characterized by the presence of distrustful cliques and factions—create political instability. *Ideological unified elites* of Communist/Fascist/Nazi varieties generate mass upheavals. *Consensual unified elites*—marked by trust, cooperation, tolerance, and moderation—make possible political democracy. *Imperfectly unified elites*, consisting of two main factions (one egalitarian, one inegalitarian), are likely to lead to political stability (pp. 35-43).

This restatement of "the elitist paradigm," Field and Higby maintain, holds "the middle ground between the classic elitist and the Marxist paradigms" (p. 19). The relations of production do have a bearing on societal conditions; so also do elites and elite conflicts.

Field and Higby proceed by (1) calling upon liberals to correct their mistakes and "recognize the essentially elitist character of their circumstances and interests" (p. 51); (2) focusing on the obligation of elitists, liberals, and others to support and promote elite consensus and unity as a means of maintaining democratic government; and (3) calling for greater freedom of action on the part of elites to handle increasingly complex world problems.

Elitism is refreshing in the unambiguous position it takes in treating a controversial topic. Some of the arguments are compelling and the book is well written. At the same time, the work is repetitious to the point of irritation and is in parts inescapably polemical. Moreover, at times the book is given to careless overexaggeration: "It is now necessary to refute the widely held assumption that values such as equality, liberty and freedom are universal and objective" (p. 3 and passim). Does a single person today accept this "assumption"? Finally, any claim to originality on the book's behalf must be tempered by a reading of Suzanne Keller's extensive analysis of "strategic elites" nearly two decades ago (*Beyond the Ruling Class: Strategic Elites in Modern Society,* Random, 1963), to which no reference is made. In fact, *Elitism* is curiously and inexplicably silent on *all* theoretical and empirical works on elites in the postwar period: with the exception of some of the authors' own writings, not a single work is cited or discussed.

MOSTAFA REJAI

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Equality, Liberty, and Perfectionism. By Vinit Haksar. (New York: Oxford University Press, 1980. Pp. 302, $24.95.)

Equality, Liberty, and Perfectionism by Vinit Haksar is a thoughtful yet disappointing work: highly priced, embarrassingly proofread and formalistic in its entry into both politics and life. Ostensibly, it is a defense of *perfectionism,* the belief that some forms of life are "intrinsically superior" to others and entitled therefore, if not to special consideration, at least to special notice. In actuality, it is a schoolmaster's demonstration that contemporary analytic and liberal theory both need and/or rely on perfectionist claims in making their case.

Vinit Haksar develops his argument in three stages. First, he criticizes attempts by John Rawls, Joel Feinberg, Stuart Hampshire, Bernard Williams, S. Benn, Phillipa Foot, H. L. A. Hart, William James and others to justify various versions of *egalitarianism* without an appeal to perfectionism on the grounds that in discussing personhood they either assume that people are (a) "wonderful creatures, compared with animals" (p. 25) or are (b) able to acquire perfectionist qualities of language, self-consciousness, autonomy, the ability to form and carry out life plans with zest, and therefore able to develop the capacity for moral sentiments and judgment.

Second, Haksar claims that perfectionist considerations must be invoked in order to work out the *liberal* implications of the egalitarian doctrine of equal respect for persons. But, he notes, the "doctrine of equal respect does not automatically commit us to a liberal society" as John Rawls and J. S. Mill would have it, nor do Rawls and Mill offer plausible explanations of why people either would (or do) choose to rank-order their values and actions. Thus, Haksar suggests, Rawls' notion that people prefer complexity to simplicity (the Aristotelian principle) and Mill's conviction that only exceptions would choose slavery over freedom meet neither real-world nor logical tests. Indeed, despite many differences between Mill and Rawls, Haksar is persuaded that the views of each are predicated on perfectionist assumptions.

Third, Haksar advances a case for perfectionism on its own merits. Using the notion of paternalism (where some people rank-order the interests of those who are not able to do so), Haksar suggests that while "some forms of life are *prima facie* more suited to human beings" (p. 295) than others, such a view does not commit one to the persecution of those who would eat their own excrement in public or otherwise engage in personally demeaning behavior. For Haksar, the perfectionist approach demands that individuals be treated as sacred ends in themselves. "What I would suggest," he concludes, "is a system of toleration but not of equal liberties for different forms of life" (p. 290).

But is it really superior and inferior "forms of life" that we need talk (philosophize) about, or rather the conditions in our shared transactional world that now threaten the human species with extinction? Haksar's mistake and the mistake of those who share his philosophical bias is that both have severed thought from its social context, substituting logic for life as the prime currency of intellectual exchange. One must wonder about the serviceability—indeed about the point—of a philosophical enterprise which seems unable to inspire wonderful creatures to act in wonderful ways or to warn them that humanity is faced with extinction through the continuing threat of thermonuclear war, environmental collapse, poverty and spreading alienation, rather than, as Haksar suggests, through people suddenly deciding not to reproduce themselves thereby bringing history to an end.

In a human and literal sense, then, Haksar's case for perfectionism is quite meaningless. It seems neither caring nor questioning enough about people's everyday lives to convey the real agony involved in anyone's or any species' premature death.

MARK READER

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On Justice: ΠΕΡΙ ΔΙΚΑΙΟϒ. By J. R. Lucas. (New York: Oxford University Press, 1980. Pp. vii + 270. $24.95.)

This is a rambling book, a series of reflections on what justice is and what justice is not. J. R. Lucas argues that there are certain prerequisites to justice. Most important is rationality. Justice is careful calculation, exemplified by the game theories he introduces in chapter 3. These calculations, though, must be limited by certain qualifications. Justice must take into account our needs; it must take into account our interests; it must take into account the needs and interests of others. It must also attend to deserts; it must attend to ties of family; it must attend to individualized characteristics. There are some things which the author argues justice is not. It is not equality; it is not utility; it is not Rawlsian.

One is left in the end with the many points to which justice must attend and yet with little, if any, guidance as to why, how and to what degree. We are told for example: "It is difficult to see the rationale for hostility to inherited wealth" and that "since we believe that the family is a good thing, it is surprising that we do not follow the principle through" (p. 248), but we are not told why the family is a good thing, why inherited wealth is necessary for the preservation of the family. One could argue seriously about both these issues, but the author appears to be content to accept common (and mostly conservative) prejudices as facts which need only be stated to be accepted. One sees a similar tendency when the author argues, still on the issue of taxes, that the wealthy Briton taxed at 98 percent should be given credit for his past contributions and not taxed again as he had been before. Lucas urges a view of justice which looks backwards "not merely to what people still have but, equally importantly, to what they have already paid" (p. 246). Yet, he wants to go back only so far. He does not want to go back to the question of how the individual reached the high tax bracket. Lucas does not clarify by what guidelines we are to decide how far back we are to go in our search for justice.

As the above examples should demonstrate, the book is bound by the British background of the author. The general reflections on the meaning of justice, suggested by the title and especially the Greek subtitle (presumably meant to recall the investigations of a Plato or an Aristotle) are limited by the perspective of an Englishman, and an English don at that. This tunnel vision is perhaps most obvious to an American reader in the author's dependence on a British idiom which begins the book and is used throughout to explain the meaning of justice: "to do down" or "to be done down by." I presume that the more graphic American idiom is "to do dirt." To rely on such a culture-bound idiom to get to the essential meaning of justice as applied to relationships within the market, within administrative units, between states, among taxpayers, etc., is to bring in serious limitations. This book is too unsystematic and too culture-bound to lead us very far in the search for an understanding of that elusive concept, justice, which, despite its elusiveness, we hold as a value to be pursued.

There may, however, be another way to read this book, one suggested by the discussion which appears on p. 60. There, Lucas is in the midst of explaining the relevance of game theory to the rationality on which he wishes to base justice. After presenting the mandatory Prisoners' Dilemma, he turns to the example of the Battle of the Sexes to illustrate the "logic of political situations" (p. 58). Lucas explains the model:

> He and She want to spend their holiday together, but he would prefer them both to go mountaineering in the Alps [a nice active pursuit, we might note], whereas she would rather they both spent it sunbathing by the sea [women always prefer the passive pursuits]. . . . If she can throw some hysterics and say she cannot abide the Alps . . . then he, if he is reasonable will . . . settle for the sea. . . . But equally he may see that the moment has come to take a firm masculine line and let the little woman face up to the realities of the situation, and either come along with him or go her separate way.

One could react by banging the book down, saying he can't do this in a book published in 1980, and by Oxford University Press, at that. He can't do this and be serious. Aha, one might think, he's not serious. He's doing this to provoke the reader, to show the reader where his or her sense of justice comes from. He's doing this to alert the reader to the dual meaning of his work, to the esoteric and exoteric content. For the average individual, happy to accept the customs and stereotypes of English society, ready to be urged towards a certain moderation from all extremes, be they extremes of the British tax law or the extremes of the "claim of the Third World for a fairer share of the earth's resources" (p. 255), for these individuals he argues that justice comes from reason tempered by a certain tenderness, and that most laws are really the result of reason and the attempt to apply rational judgments to specific situations. But for those not quite so ready to settle into the easy-going acceptance of society as it currently exists, these are the ones he may want to arouse; he may want to show that justice is best explained, as Aristotle pointed out (*Nichomachean Ethics* V. 1), by reference to what is unjust. (Perhaps it is this which explains the Greek subtitle.) Lucas may want to show that justice does not come from the careful calculations inherent in a game theory model, or from weighting arguments and evidence in the mythologized courtroom, free from rhetoric and appeals to emotion which he paints on pp. 36 and 80. Rather, justice comes from ire, *thumos,* a sense of what is wrong with the assumptions and biases of contemporary society. The key to Lucas' real intent may be on p. 60. Only the number of chapters does not add up to thirteen.

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Improving Policy Analysis. Edited by Stuart S. Nagel. (Beverly Hills, Calif.: Sage Publications, 1980. Pp. 264. $18.95, cloth; $8.95, paper.)

Literature on policy analysis abounds—one is continuously inundated with works which analyze various stages of the policy-making process, identify methodologies and approaches for doing policy analysis, and even examine the substantive nature of policy and politics. This volume adds to the proliferation of this literature by attempting to improve our ability to determine what policies will achieve goals and to determine the effects of policies (p. 7). Does it strengthen our knowledge of policy analysis? The answer is a definite yes. The 13 contributions to the volume are generally well done and, in total, provide a comprehensive survey of the state-of-the-art of policy analysis. Although it suffers from the lack of a concluding chapter which would integrate the important ideas found within, this work would be a valuable contribution to graduate readings and it is recommended for anyone interested in *doing* policy analysis.

The work is divided into four parts: (1) deductive modeling and optimizing in policy analysis; (2) the legal perspective in policy analysis; (3) the evaluation research perspective on policy analysis; and (4) implementing policy analysis research. Although each of these sections is valuable by itself, the book is much more important for several central issues raised about policy analysis. Among them are the nature of policy analysis as a profession, appropriate methodologies, and utilization.

An important question about policy analysis is its place in social science research—is it a fad or a field of study worthy of institutionalization? Although only a few authors address this question explicitly, by implication it is addressed in virtually every article. Widespread agreement emerges that policy analysis can effectively contribute to better policy making, that there is a developing set of methods and approaches for doing policy analysis, and that a rapidly growing professionalism is evident. The two most useful articles addressing this concern are by Duncan MacRae ("Policy Analysis Methods and Governmental Functions") and Arnold Meltsner ("Creating a Policy Analysis Profession"). Meltsner states that "It is not unreasonable to assume that policy analysis . . . is a more or less permanent feature of public policy making" (p. 236). One finds very little disagreement on this point throughout the volume.

On other issues, more disagreement is evident. For example, a persistent question concerns the methodology of policy analysis. In fact, some contributions approach policy analysis only as a methodology. John Brandl ("Satisficing, Feedback, and Decision Rules in Government") and Werner Hirsch ("Policy Analysis of Legal Policies") are most indicative of this perspective, which seems to value measurement precision and formal modeling to the exclusion of other elements of policy analysis. In contrast to this perspective, and more convincingly demonstrated in this volume, is a perspective which characterizes policy analysis as including "much more than procedures for gathering and statistical manipulation of data" (MacRae, p. 130). This broader conception includes attention to qualitative approaches, attention to political feasibility and administration (see Nagel, "Political Science and Public Administration as Key Elements in Policy Analysis"), and to designing information systems to meet user needs. In this regard, the contribution by Peter House and Joseph Coleman ("Realities of Public Policy Analysis") provides an excellent overview of the practical considerations of doing policy analysis research, and Jack Byrd's refreshing article ("The Humanization of Policy Models") advocates a strong dose of common sense in building policy models.

The most significant contribution of the volume is the consideration given to the use of policy analysis. While some authors avoid this issue or simplistically assume that use is related to the identification of causal relationships, most of the contributions emphasize what is done with policy analysis rather than how policy analysis is done. In this respect, particularly interesting questions are raised about the sociology of knowledge (Edward Lehman, "Policy Research: Industry or Social Movement") and about social scientists' perceptions of barriers to utilization (Karin Knorr, "The Gap Between Knowledge and Policy"). No easy conclusions can be drawn about utilization. However, the clear picture from this volume is that policy analysts can contribute to positive social change, and that the responsibility to understand and enhance the utilization process lies with the researcher.

STEVEN BALLARD

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History of the Idea of Progress. By Robert Nisbet. (New York: Basic Books, 1980. Pp. xii + 370. $16.95.)

Robert Nisbet has written not a history of the idea of progress but a comprehensive survey of Western thought about "progress," however variously understood. His book is not guided by any interpretive rationale or philosophical reference point which might serve to distinguish one level of meaning from another. Even in the epilogue, where an attempt is made to draw conclusions from the survey, Nisbet preserves a neutrality which respects all theories encountered on this guided tour of the reliquary of great books which mention or imply some notion of "progress" (from Hesiod to Teilhard de Chardin). Rather than working out a critical analysis of these ideas about progress, the author is satisfied to provide a kind of annotated bibliography or catalogue of passages which he believes will demonstrate that Western civilization has been supported throughout its career by "a rarely interrupted conviction that the very nature of knowledge . . . is to advance, to improve, to become more perfect" and that the goal of this advancement is an "ever greater perfection of human nature" (p. 5).

While never in any age "universally held by intellectuals" (p. 6), progress is always—at least until quite recently—"the dogma" toward which "the overwhelming majority of the greatest minds in Western history are oriented" (p. 8). Since Nisbet believes this orientation was a positive force in the development of civilization, he worries about the consequences of the rapid waning which "everything now suggests" the dogma of progress is experiencing "in all levels and spheres in this final part of the twentieth century" (p. 9).

Quite understandably, Nisbet encounters expressions which evoke some notion of "progress" just about everywhere in Western literature. Thus his account of the history of these expressions has a cast of hundreds, headed (in order of the attention they receive) by St. Augustine, Marx, Comte, Turgot, Condorcet, Plato, Herbert Spencer, Hegel, Darwin, Saint-Simon, Rousseau, Joachim de Fiore, J. S. Mill, Adam Smith, and Lucretius. But his litanies of names and concepts, his frequent citations and quotations (all without the documentation one expects in a work of scholarship) advance no thesis. As he admits in the foreword, the only thing Nisbet tries to do is "identify and put in proper perspective the major personages, texts, presuppositions, intellectual climates, and philosophical and ideological uses of the idea during the past twenty-five hundred years" (p. ix). Apart from any bibliographical purpose this might serve, it is not clear how the author expects us to profit from his survey (although he thanks his sponsors at the American Enterprise Institute "for their perception of the book's subject as falling within public policy research" [p. xi]). His frequent lamentations about the lack of space to develop some theme or other suggest that Nisbet himself felt frustrated by his project.

Whatever its aim may be, his history reads like a well-informed conversation one could imagine overhearing at a faculty club, where leisurely relaxation rather than serious scholarship is in progress. Among the hazards of generalization to which the book succumbs, the most serious is its failure to make the necessary critical distinctions about the different meanings of "progress" and their significance for the history of ideas. Just about any kind of positive development or process, any sense of progression in time, any theory of continuity or advancement is sufficient to count as "progress," whether it be human or divine in inspiration, temporal or spiritual in content, physical or moral in effect. Even the Aristotelian doctrine about the movement in nature from potency to act is an idea of "progress."

Of the consequences of this lack of critical precision, a few examples must suffice: Nisbet detects no essential difference between the Augustinian and Hegelian philosophies of history; he misses the crucial contrast among the American Founders between Adams and Jefferson; he treats as equally "Christian" orthodox forms of belief and such aberrations as millennialism.

At the very least, definitional responsibility would seem to call for a clarification of the difference between a sense of progressive development and the presumption of progress (progressivism) one finds in the political ideologies which are the product of modernity in the history of philosophy.

The book is divided into two large chronological parts (before and after 1750). Part 1 focuses on the classical, early Christian, Medieval, Renaissance, and early modern periods. Although Nisbet regards this part as his most original contribution to the literature on progress (in contrst to J. B. Bury's denial that any positive attitudes toward progress could be found in the premodern period), it is principally here that a lack of definitional precision creates the most problems. Plato, Aristotle, Augustine, and Dante are all made out to be partisans of temporal progress in spite of the manifest orientation toward permanence rather than change which pervades their thought. On the other extreme, Nisbet fails to discover the progressive spirit of Erasmus, Machiavelli, Bacon, and Descartes which so powerfully helped to lay the foundations of modernist progressivism.

Part 2 is more persuasive; the progressive spirit that prevailed after 1750 would be hard to miss. Here Nisbet drops the chronological format and organizes his material around two "tests" which reveal the modern orientation toward progress: the expansion of freedom and of power.

Having reached the twentieth century, Nisbet closes his survey with contrasting essays on the contemporary persistence of progress and the intellectual questioning of that idea which he finds unique to our own times. The suggestion that our century is unlike all that have preceded it in this respect is weakened by the same flaw which pervades the whole work: "progress" is seen as a general idea of virtually limitless range rather than as a spirit with precise philosophical or ideological tendency against which objections were raised in all times and places.

JOHN A. GUEGUEN

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The Laws of Plato. Translated, with Notes and an Interpretive Essay, by Thomas L. Pangle. (New York: Basic Books, 1980. Pp. xiv + 562. $26.50, cloth; $11.95, paper.)

Some years ago Leo Strauss put forth the claim that the Platonic dialogues are artistic masterpieces which cannot possibly be understood unless account is taken of their dramatic form and attention paid to all of their details, even those seemingly trivial and obscure. One implication of this claim, which Strauss' commentaries have gone a long way to substantiate, is that literal translations of the dialogues are absolutely essential. Strauss himself published no translations, but his students have, several in recent years. Of these the most recent and, indeed, the most monumental is Thomas Pangle's translation of the *Laws*.

Pangle's achievement is remarkable: He has provided as accurate a translation of the Greek as readable English prose will allow; and since the only other translation of the *Laws* based on Des Place's and Dies' edition of the Greek text (the Penguin *Laws* by Trevor Saunders, 1970) is confessedly mistranslated, Pangle's book is especially needed and welcomed.

The accompanying interpretive essay is an excellent distillation of a dialogue three times its size. The commentary is thoughtful, even profound; and it amply demonstrates the importance of reading Plato carefully and from a translation that is true to his language.

Pangle begins by noting the strangeness of the *Laws* to contemporary liberal views. The source of this strangeness seems to lie in the dialogue's preoccupation with the problem of politics and philosophy. Because liberalism treats politics as the reconciliation of competing interests, it cannot quite comprehend politics as moral education; nor can it see this education as dependent on, as well as threatened by, philosophy: to restrain material gratification and the free expression of ideas is intolerance serving the cause of totalitarianism. But liberalism today suffers from a lack of confidence, and it is at all times loath to defend itself. Nothing would better help to clarify and reinvigorate liberalism than for its proponents to study Plato's critique—because it is a qualified critique equally aware of the objections to political utopianism.

Much of Pangle's analysis centers around the question of the contribution of political order to human happiness: does Magnesia, the city of the *Laws,* offer the best life *attainable* by human beings? In light of a "Rousseauian" state of nature present in book 3 (primitive man as simple and innocent), civil society would seem able to justify itself "only on the basis of the hypothesis that man is mutilated without the pursuit of wisdom" (p. 427). Philosophy vindicates the sacrifices involved in political virtue. But few men are philosophical; and since the knowledge that is a concomitant of civilization empowers human beings to injure one another, politics must become increasingly repressive in order to control this potential for evil. The goodnes of political life is fraught with ambiguity if a sub-political life were thought to be decent (a "Hobbesian" state of nature inhabited by Cyclops is also suggested) and a trans-political life beyond the reach of most men.

Pangle explains the early books of the *Laws* as an attempt to find a suitable and effective foundation for moral virtue. What he terms the "psychology of citizenship" is rooted in various conceptions of virtue and happiness and is benefited by a number of external supports. First, there is courage understood as heroic struggle pointing to the pleasures of freedom. Spirited citizens find the rewards of political participation especially appealing and are devoted to the city on this account. In a similar manner, the founder is tied to the justice of the city by his pursuit of personal fame. A sense of shame giving rise to moderation can be an alternate basis of political virtue. This is assisted by a pious poetry that tells of just and benevolent gods. There are also the political institutions of a mixed regime which lend support of an artificial kind. But the city's grandest offerings, short of philosophy, are the intellectual and artistic pleasures of music. The aesthetic soul is a sort of intermediary between the courageous and the moderate on the one hand and the philosophic on the other.

Notwithstanding these advantages and pleasures, Pangle understands the *Laws* to say that political virtue, if it is not to become a means to an end lower than itself, must point to something higher than itself. The city cannot philosophize, but it needs philosophy in order to justify its moral education; more practically, it needs the wisdom of the philosopher to supervise the changes of law. But the persistent question is whether the city's need for philosophy is compatible with its need for piety. The *Laws* offers some resolution by using philosophy as a defender of the faith and by subjecting the impious to the "punishment" of philosophic dispute. That a place for philosophy can be found within the city is suggested by the willingness of Kleinias and Megillus to subordinate themselves to the Athenian Stranger.

PATRICK COBY

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Political Theory and Political Education. Edited by Melvin Richter. (Princeton, N.J.: Princeton University Press, 1980. Pp. 203 $13.50.)

This book is an edited volume of previously unpublished papers by prominent political theorists which were first presented at an international meeting of the Conference for the Study of Political Thought. The questions addressed by the essayists are suggested by the title: how should the next generation of political theorists be trained; how should they understand their vocation; and what can political theory contribute to the education of nonacademics, such as citizens, lawyers, and officeholders?

The contributors and their essays are: Bernard Williams, "Political Philosophy and the Analytical Tradition"; Charles Taylor, "The Philosophy of the Social Sciences"; Mihailo Markovic, "Marxism as a Political Philosophy"; Allan Bloom, "The Study of Texts"; J. G. A. Pocock, "Political Ideas as Historical Events: Political Philosophers as Historical Actors"; Michael Walzer, "Political Decision-Making and Political Education"; Ronald Dworkin, "Political Theory and Legal Education"; and Sheldon Wolin, "Political Theory and Political Commentary."

Like most volumes of conference papers, the book's promise somewhat exceeds its performance. The essays, as a group, are of excellent quality but are rather sketchy. They provoke and tantalize more than they satisfy. Nevertheless, all who are seriously interested in contemporary political theory will find reading this book a rewarding experience—for a number of reasons.

First, it provides a very good overview of the contemporary rebirth of political theory. As Melvin Richter's extensive introduction makes clear, each of the contributors represents in his own work an important facet of this rebirth. Reversing the widespread impression of a couple of decades ago that political theory exhausted itself in the collection of philosophically obsolescent museum pieces, the volume demonstrates the mutually informative relationship between political theory and several contemporary intellectual disciplines and developments. These points of tangency can be expected to be increasingly significant in the next decade; and those who intend to work in political theory should be conversant with them.

Next, the essays help to exemplify some of the widely (though by no means universally) shared views of most political theorists today—for example, as to the intellectual inadequacy of positivism and the moral insufficiency of utilitarianism. At the same time, some of the more interesting and significant disagreements are also explored—for example, in the different recommendations of Bloom and Pocock for studying the classic texts.

Above all, the essays offer in their distinctive ways some very provocative intimations of ways to accomplish a major task of political theory: to provide illumination, perspective, and clarity for citizens and political leaders confronting the choices and decisions in their own lives and vocations.

THOMAS A. SPRAGENS, JR.

Duke University

Collective Decision Making: Applications from Public Choice Theory. Edited by Clifford S. Russell. (Baltimore: Johns Hopkins University Press, 1979. Pp. xvi + 296. $24.00.)

These papers, originally presented at a Resources for the Future forum, aim to demonstrate that public choice theory can explain real political action and guide actual policy decisions. Lamentably, this aim is unmet. Of course, public choice literature elsewhere does contain outstanding accounts of real political behavior, e.g., Morris P. Fiorina's *Congress: Keystone of the Washington Establishment* (Yale, 1977), and William A. Niskanen's *Bureaucracy and Representative Government* (Aldine-Atherton, 1971). However, the contributions in this volume will, by and large, feed the fear that public choice research is "the highly theoretical plaything of a few intellectuals" (p. xvi), as Hitch puts it in his introductory remarks. The bulk of the articles fall into three basic categories: (1) nonexperimental research drawing on the self-interested, rational actor hypothesis, e.g., the papers by Portney and Sonstelie, and Mitchell; (2) laboratory experiments on decision-making rules, e.g., the papers of Plott, and Ferejohn, Forsythe, and Noll; and (3) review of policy applications of public choice, e.g., Tullock's paper. (Each of these articles is followed by comments from other participants in the forum.)

The piece by Portney and Sonstelie on "super-rationality" and school tax voting typifies too much of the empirical work in the public choice field. That is, one must strain to believe the rational (in this case "super-rational") calculations the actors are assumed to carry out in making their decisions. Here the assumption is that, in a school tax election, home owners vote to maximize the value of their property. As Russell points out in his introduction, this requires one to act "as if one knew the market's (other people's) evaluation of marginal changes in unmarketed services" (p. 11). Such an assumption is hard to swallow. Mitchell's paper tries to explain why individuals join environmental lobbies like the Sierra Club when, according to Mancur Olson's logic, they could receive the group benefits without joining. Hardin's comments imply—rightly, in my view—that Mitchell's approach is wrong-headed, for it tries to explain the exceptional case. For instance, a million Americans do belong to environmental groups, but over 200 million do not, perhaps because they are free riders à la Olson.

In the editor's opinion, the papers of Plott and of Ferejohn et al. are "the most exciting of the forum" (p. 14). I must dissent from this judgment. Plott carries out laboratory experiments intended to test alternate theories of decision making under majority rule. Ferejohn et al. explore, in the lab and in practice, how public television stations allocate their budgets to acquire programs. While these experimental efforts are quite sophisticated, they are poor examples of *applied* public choice research. With Chamberlin, I worry over their external validity. They certainly do not meet his demand that "the experimental conditions be sufficiently like the conditions in the naturally occurring environment" (p. 162). Rather, like most of the work in axiomatic social choice theory, it is heavy with unreality.

We come to the final concern of the volume, which is to show the influence of public choice theory on governmental policies. To summarize the principal conclusion on that score, let me simply quote Tullock: "If . . . we look for specific actual changes in government policy which can be directly attributed to public choice activity, I think we would find that there are none" (p. 33).

Why does this set of papers not provide convincing applications of public choice theory? One reason, I would speculate, is that many of the authors have little concern for (understanding of?) political realities. Like the reformers of the Progressive era, they believe all we need for good government is better rules. Tullock justly criticizes those reformers whose only remedy "was more careful selection of personnel and moral lectures" (p. 31). But how far is this naive remedy, at least in spirit, from the institutional design proposals of many public choice theorists? The moral tone is shared, as evidenced by Plott's declaration: "Social organization and institutions must be 'correct,' in order for the 'proper' behavior to automatically [sic] flow from the public sector" (p. 137). Further, there is boyish optimism about the ease of implementing their proposals. For instance, Ferejohn et al. remark, "Many if not all of the features of economic environments can be modeled easily and usefully in laboratory settings" (p. 188). And, Tullock opines, "If the present trends are in the direction of greater centralization, it still seems to me that it would not be very difficult to reverse them. . ." (p. 42). But it is not easy to introduce new political rules, even if they are "correct." Haefele, in his contribution, notes that Jefferson wrote a parliamentary manual for the Senate, which is very like a set of rules from contemporary social choice theory, but very unlike the current rules of the Senate. An appreciation of Jefferson's failure to make his rules stick should temper the self-assurance of these modern-day "institutional engineers" (see p. 139).

MICHAEL S. LEWIS-BECK

University of Iowa

Marxism, Socialism, Freedom: Towards a General Democratic Theory of Labour-Managed Systems. By Radoslav Selucký. (New York: St. Martin's Press, 1979. Pp. xiii + 237. $22.50.)

Radoslav Selucký's provocative book deserves careful reading. He claims to have discovered, through his reexamination of Marx's concept of the market, the theoretical mistake which turned Marx's ideal society into a nightmare; to prove the impossibility of a non-market democratic theory; and to have so redefined socialism that, "stripped of its utopian features, enriched by valuable liberal principles and based on a synthesis of the market and social planning, it is possible to construct a theoretical model combining socialism, individual freedom and political democracy, as well as a democratic theory maximizing both sosocial welfare and liberty" (pp. xi-xii).

In the course of a very interesting argument, Selucký suggests that, even given the distribution of wealth according to labor output necessitated by scarcity and the social division of labor, a (socialist) command economy adds to the alienations historically associated with capitalism the further alienation of workers from their wages. Since "the demand for the desired use-values cannot be identical with the sum of the fulfilled plan targets . . . any consumer's attempt to promote his needs through effective demand is irrational. . ." (p. 39). One structural consequence of the frustration of effective demand typical of non-market, socialist systems is the emergence of a potent, ramifying black market.

History, reason, common sense, and experience have shown the market to be essential to insuring efficient production. Furthermore, not only does the market express and preserve the autonomy of the individual, but also structural links exist between the market and positive freedom. For Selucký, the preservation of the market is the necessary condition for securing in socialism five "ethically valuable liberal principles": (1) negative freedom, (2) the right of every individual to pursue his or her concept of happiness, (3) the right of every individual "to develop all his potential talents, abilities and faculties," (4) the equality of all persons, and (5) the right of the individual "to decide about the matters which have an impact upon" his or her life (pp. 173-75).

In an acute analysis of the works of Milton Friedman and C. B. Macpherson, Selucký demonstrates that (1) a (regulated) market can exist in socialism and (2) "had Macpherson realized that a market under certain circumstances could protect the negative liberty of the many *and* yet bring about some positive liberty to the many, he would certainly be less categorical as to his condemnation of the market and his praise of non-market libertarianism" (p. 151). Like Friedman, most people identify a market economy with competitive capitalism, while "socialism is identified with a non-market economy relying on coercive macroeconomic coordination" (p. 136). But such identifications are possible because those making them fail to "distinguish between the essence (market) and the appearance (competitive capitalist market), between the general (autonomous enterprise) and the particular (private enterprise)" (p. 137).

Given capitalist premises, the market causes contradictory social effects because "individual producers verify the social usefulness of their labor . . . through an intermediary, . . . only when the product has reached the market" (p. 5). Thus it tends to cause disharmony between supply and demand, it increases social inequality, "it encourages only economic activity which yields profits and is advantageous for the producers," and it leads to monopoly (p. 6). Because the market is a mechanism of production necessitated by scarcity and the social division of labor, phenomena which will survive a transition to any foreseeable socialist system, its dysfunctions can best be eliminated or mitigated by subordinating it to socialist premises by extending liberal democratic principles to the economy itself. These socialist premises are: (a) public (rather than state) ownership of the means of production following the Yugoslav innovation; (b) the establishment of an (indicative) social plan, exempting from market determination health and education needs; (c) distribution according to labor output; and (d) the extension of equality to the economic sphere. "Public ownership and social planning . . . acquire socialist properties *only* if combined with distribution according to work and equality extended to the economic sphere *provided that* principle (a) implies equal access to social capital, (b) implies social security and welfare, (c) implies absence of any exploitation, and (d) implies equal distribution of, and equal access to, economic and political control" (pp. 137-38).

One may wish that, in the course of his generally sound critique of Marx, Selucký had adverted to the works of Agnes Heller, and had concentrated on the distinction between single and complex labor and on the differences among natural, necessary and radical needs. By adding Heller's insights to his own, Selucký could have made his argument—that Marx's failure to understand the necessity for the market (following even the elimination of value from the second phase of communism) led to Lenin's statist excesses—more powerful and more subtle. More personally, I would like to know what the author might have made of Jean Baechler's arguments (1) that private property is the *sine qua non* of individual autonomy, and (2) that democracy is the source of capitalism; not the reverse, as Selucký argues.

W. R. CAMPBELL

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The Rights of Reason: A Study of Kant's Philosophy and Politics. By Susan Meld Shell. (Buffalo, N.Y.: University of Toronto Press, 1980. Pp. xi + 205. $13.50.)

The Rights of Reason is an important, original, and penetrating study of Kant's philosophy and political theory in light of his understanding of the relation between man and nature and its bearing on his philosophy of right. It is the first sustained attempt to explore these dimensions of Kant's thought; moreover, Susan Shell's approach demonstrates a degree of coherence to Kant's view that is lacking in other accounts. In successive chapters, Shell considers Kant's early writings on nature, the development of critical philosophy, the growing emphasis on morality, and his doctrine of right. This allows her to show the development of Kant's thought, but also the persistence of certain basic themes, from the early writings in which Kant viewed nature as indifferent to human ends and needs, an assault on both our dignity and security, to the last essays on man's prospects in a world of strife and injustice. Thus the basic categories—nature, reason, morality, and law, and the subtle interrelationships among them—appear throughout the work. The idea of property also looms large in this study; Shell views Kant's metaphysics, jurisprudence, and doctrine of right as substantially theories of property, answers to the questions, what can I legitimately claim as my own and on what basis? Property, for Kant, is thus not so much a source of satisfaction as a "condition of rational thought and action" (p. 187). Moreover, with the possession of any right, there is always for Kant a limitation to be accepted. Shell does not give us a comprehensive account of Kant's political philosophy, but she does provide the framework within such a task might be attempted.

Kant's moral philosophy is the usual avenue into his political theory; Shell believes, to the contrary, that we must begin with nature. Nature as opposition and man's righteous reactions to it, she argues, define the basis of Kant's thought. "Our concept of right is initially awakened by natural objects which push against the will and resist it. Right is itself a revolt against nature, a revolt in which no other creatures share" (p. 113). As children we react in anger to whatever inhibits our will; we claim rights against nature itself, and against other persons. Moreover, reason itself develops through opposition. It is through the sense of touch, for example, that we develop the notion of form, and abstraction, with its distancing, involves a repulsion of nature. Similarly, morality develops in opposition to an indifferent nature, one that frustrates and harms us. Yet only by turning that anger inward and accepting limits on one's desires can we begin to overcome nature. Moral experience, for Kant, is "inevitably jarring and disjointed. Morality draws us up against nature, . . . especially against our own nature" (p. 67). With morality and a sense of right, a fundamental distinction appears, between willed and unwilled opposition, between the opposition of persons and the forces of nature. When anger is turned outward, it will now be against our fellow beings, as indignation. Blame, however, involves an impersonal standard; with blame comes rights and a recognition of equality. "Right, properly understood, is rational indignation, anger objectified" (p. 110). Henceforth, "right and duty, self-assertion and self-limitation . . . go hand in hand" (p. 110). Property, law, and political order also spring from opposition, coming mainly from our fellows, but become aspects of a coherent doctrine of right, the purpose of which is to provide the principles necessary for reconciling competing claims. Yet, wherever persons and nature (and not just self and others) meet—reason, morality, property, or law—there are inevitable limitations that must be accepted. To be free is not necessarily to be happy; that is something we can hope for, and to labor for, but it is not ours as a matter of right. Kant would have rejected the optimistic expectations of Hegel and Marx as "dangerously mystical and misguided," but he did not glory "in deprivation" (p. 4).

Susan Shell has written a fine book, but not one free from certain difficulties. To begin with, she sets up expectations that are not fulfilled. In the introduction, Shell indicates that she will offer criticisms of Kant's conceptions of "nature and justice and their interrelation," and also that her book will shed "new and useful light on Kant's place in the history of liberal thought" (p. 5). Her ideas with respect to both are suggestive, but not developed, and occupy only two pages of the text. Also, her explication of Kant is far too compressed, even, at times, elliptical. Her comparison of Nietzsche with Kant (p. 122) rests on a confusion: she mistakenly equates revenge (*Rache*) with *ressentiment*. More seriously, she *assumes* that Kant is a contract theorist, yet her own evidence from Kant seems to argue against this (p. 168). She also states that Kant's theory of political obligation rests upon consent, but this is simply not the case. Finally, though she provides evidence of a Kant quite different from the one most of us had imagined—a Kant who has looked into the abyss, who has observed deeply the tragedy of man in nature and society, and yet retained hope—she gets no further than describing him as "melancholy," meaning one who is "righteous and embittered by injustice" (p. 179). There is, nevertheless, in the material Shell presents a Kant who is as aware as Pascal, Nietzsche, or Kafka of man's existential dilemmas and vulnerabilities. Kant does not move beyond good and evil, but he sees beyond them. To her credit, Shell points in the direction of this Kant, even though she leaves the portrait unfinished.

ROGER W. SMITH

College of William and Mary

Knowledge and Decisions. By Thomas Sowell. (New York: Basic Books, 1980). Pp. x + 422. $18.50.)

"Every false diagnosis of a condition," Thomas Sowell remarks (pp. 258-59), "is an obstacle to improvement." Unfortunately, he ignores his own good advice too often, with the result that an impressive effort to diagnose the modern condition is badly weakened by questionable assumptions, shotgun empiricism, and ideological caricature.

An economist of remarkable range, who is as well prepared to analyze civil rights law as he is anti-trust policy, Sowell writes in the tradition of neoclassical liberalism—a tradition now firmly transplanted from Manchester and Vienna to Hyde Park and Westwood. He criticizes virtually all state intervention in the economy, except in the case of "natural monopolies" (following Milton Friedman) or when third parties are adversely affected by a transaction. Like Friedrich Hayek, he identifies the central issue of modern times as the conflict between the freedom anchored in the market economy and the coercion exercised and threatened by all state authority, in its most extreme form by totalitarian regimes.

According to Sowell, to defend the "freedom of the many" (p. 289) is to go against the grain. Intellectuals have always been apologists for despotism (p. 349) and the "presumed wisdom of the few" (p. 289). Today they form a rapidly proliferating "new class" whose aim is to use the force of the state to wrest power from the business elite. By now, this is a claim familiar in the work of "neoconservatives," but Sowell tries to give it comprehensive and theoretical statement.

Sowell's premise is that decision making requires knowledge, defined broadly to include all ideas requiring authentication. Social decisions are made with the "social knowledge" conveyed in economic, political, and social transactions. This conception of knowledge blends communication, organization, and economic theory to stress the transmission of information (variously authenticated) about costs and benefits in a decisional context of systemic incentives and constraints.

The thesis is that the system of voluntary exchange among self-interested parties through the price mechanism (alias capitalism) is a more socially efficient process of decision making than that which depends on government in all its forms. Economic actors directly affected by their own decisions are held better able to calculate costs and benefits than remote politicians and bureaucrats; "surrogates" (i.e., representatives), administrators, and judges interpose their own self-interest, because their power and reward batten on the enlargement of their roles. Public policy is further distorted because collaboration develops between the regulators and the special interests who become their clients and constituents.

The general point was first made by Adam Smith, as Sowell recognizes. Here it is cast in different terms and illustrated by contemporary examples. The more remote the decision maker, the higher the "knowledge costs." Money prices, because they are "fungible," allow for versatility and adjustment, whereas government policies must be categorical rather than incremental (a notion that may surprise most students of budgeting and policy making) and require articulation "documentable to third parties" (p. 40, and passim) in such forms as legal briefs and decisions, environmental impact statements, and compliance reports. In economic transactions, consumers and investors can estimate cost-benefit ratios effectively, but as taxpayers and citizens they are in a poorer position to do so. Special interests are better able to pay the knowledge costs and to capture the benefits of political decisions for themselves while the costs are widely diffused. Whereas the market imposes severe penalties for miscalculation, political outcomes are often long-delayed, and bad outcomes are not necessarily costly to those responsible for them: politicians can avoid accountability and administrative agencies and courts are insulated from political feedback.

It is because of the trend away from reliance on the market in favor of a greater role for centralized government, Sowell asserts, that the political landscape is strewn with such wreckages of public policy as urban renewal, public housing, unemployment (because of minimum wage laws), school integration, and unwarranted subsidies to farmers, shippers, et al. All these fiascos and impositions are systemic failures, resulting from an increasingly inefficient coupling of knowledge and decision making—or, in other words, from the substitution of government for the free market.

One need not deny that American government has weaknesses contributing to failures of policy to find Sowell's analysis of the causes of and remedies for these problems incomplete and one-sided. A full critique of his collage of arguments and examples would require at the very least a lengthy summary of much that has been written about the economic and moral deficiencies of unregulated capitalism, and the need for state intervention to maintain economic stability and assure equal freedom in social relations. For present purposes, a few key points must suffice.

The notion that market transactions are voluntary and therefore not coercive conveniently ignores the embeddedness of private property and inequalities of wealth in a system of political sanctions. If such sanctions are coercive, as Sowell claims they are, then so are the property relationships they enforce. Monopolies and oligopolies, moreover, enjoy a "market power" (to set prices, influence tastes, limit options, and affect community welfare) which, contrary to Sowell (p. 118), is not merely a metaphorical form of coercion.

Nor is government coercion necessarily antithetical to freedom. In a constitutional democracy, coercion may be used only to enforce laws collectively self-imposed. The right to participate in the making of these laws and the right to be shielded against abuses in their application emanate from the same underlying commitment to freedom that allows for market transactions. The coercion exercised by totalitarian regimes is wholly different in character, because it is not authorized by laws freely adopted by the citizenry and because its use is not limited by any guarantees of individual rights. Sowell's claim that the difference between totalitarian and democratic regimes is in the degree of coercion practiced obscures this vital qualitative difference and the practical consequences flowing from it.

Many of the examples Sowell cites illustrate only half the truth at most. To insist that apart from states only trade unions "often use force, threats and harassment" (p. 175) is to indulge a bias: the same could be said of businesses involved in labor disputes. The political insulation of courts and administrative agencies is a problem in some respects, but the independence and impartiality it promotes are socially beneficial, as the framers of the Constitution anticipated. Nor is it wholly correct to claim that regulatory agencies are invariably and entirely the captives of their clients or that they are immune from criticism—witness the recent harsh attacks in Congress against the Federal Trade Commission for stepping on the toes of too many business people.

And what of the role of intellectuals in the past, and of the "new class" in the supposed conflict between the despotic state and the free market? As to the past, Sowell's tendentious history of intellectuals loses a legion of stalwart libertarians, including Cicero, Milton, Locke, the Mills, de Tocqueville, Thoreau, and Adam Smith himself, but manages to include Adolf Hitler (p. 352). With respect to the present, his indictment concentrates on the lucubrations of left-wing literati and ignores the impact of such suppliers of policy-relevant knowledge as the Keynesian technocrats (econocrats?), defense strategists, policy analysts, and engineers and natural scientists, whose work has become increasingly influential in the making of foreign and domestic policy, and who are hardly all Bolsheviks. If all academics are counted as intellectuals, a picture very different from Sowell's emerges from survey data. A 1977 poll by E. C. Ladd and S. M. Lipset (Lipset, "The New Class and the Professoriate," *Society,* Jan.-Feb. 1979, pp. 31-38) showed that 81 percent of all American faculty members favor free enterprise and 69 percent fear the expansion of the role of government. Most of the "new class," it would appear, is not exactly ready to rush to the barricades.

For old and new conservatives, then, this book will serve as a heady highbrow tract. For others, including myself, who have learned to respect Sowell's commitment to honesty and realism, it is a disappointment because its diagnosis is so faulty as to be, in his own words, "an obstacle to improvement."

SANFORD A. LAKOFF

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Naturalism and Social Science: A Post-Empiricist Philosophy of Social Science. By David Thomas. (Cambridge: Cambridge University Press, 1979. Pp. vii + 213. $32.50, cloth; $11.50, paper.)

Naturalism—the doctrine that social phenomena are in certain respects like natural phenomena and can be explained in much the same fashion—has not wanted for critics. In the late nineteenth century Dilthey and Neo-Kantians like Windelband held that the *Geisteswissenschaften* (a translator's rendering of J. S. Mill's "moral sciences" into German), having sentient subjects as "objects" of investigation, required a distinctive method, that of "understanding" or *Verstehen.* This view, which can be traced back to Vico and Herder, was later adopted and refined by neo-idealists like Collingwood and Dray, and Wittgensteinian "ordinary language" philosophers like Peter Winch.

Anti-naturalism is not without its own problems. How, for example, might we understand the actions and practices of people who appear to systematically misunderstand themselves and their situation? (This is of course the old Marxian problem of "false consciousness.") Moreover, how can we begin to explain someone's behavior "in his own terms" if the very description he supplies—that he is "exorcising demons" or "making rain"—is suspect or patently false? And, if we seek to explain behavior in terms of the agent's own reasons, aims and intentions, how can we hope to account for the unintended consequences of purposive action? Questions like these prompted Weber to attempt to synthesize naturalist and *verstehende* approaches, and his lead has since been followed to good effect by G. H. Von Wright, Jürgen Habermas, and Donald Moon (see his contribution to the *Handbook of Political Science,* eds. Polsby and Greenstein [Addison-Wesley, 1975] and his and Brian Fay's "What Would an Adequate Philosophy of Social Science Look like?" *Philosophy of the Social Sciences,* Sept. 1977).

Unfortunately, these recent attempts at synthesis get short shrift in David Thomas' *Naturalism and Social Science,* where they are dismissed or ignored altogether. Briefly, Thomas wishes to defend a "post-empiricist philosophy of social science" based upon new developments in the philosophy of the natural sciences. This new approach recognizes the importance of certain concepts and categories—e.g., "meaning"—which were virtually ignored by the older "empiricist" (or positivist) approach. But Thomas maintains that "meaning" is not the exclusive province of the agents whose behavior is being investigated; it belongs also, and more importantly, to the inquirers who, with the aid of one or another theory, conduct their investigations according to scientific criteria of significance or meaningfulness. It is not enough, he argues, to provide a culturally or contextually meaningful account of an action or practice; the social scientist's task is to render it scientifically meaningful via a trans-cultural theory of (say) social structure. The latter enterprise takes precedence over the former: "the observer must ultimately be prepared to impose his conceptual framework on the objects and actors under study" (p. 106). Thomas reaches this conclusion by way of a series of critiques of anti-naturalist arguments advanced by Winch and others. As he shows, even ostensible anti-naturalists like Winch require some sort of trans-contextual interpretive framework or theory. Common-sense "understanding" of "meaning" is no substitute for a theoretically grounded scientific understanding of social phenomena.

Thomas' book is rich in philosophical argumentation and in apt social-scientific illustration. Yet it is an uneven book, lucid in places and densely opaque in others (much of it still reads like the dissertation it once was). Moreover, Thomas makes things too easy for hiself by retreading, however intelligently, well-worn ground. Surely earlier interpretivists like Winch and Collingwood are easier targets than are later "hermeneutic" theorists like Hans-Georg Gadamer—which, incidentally, is why Habermas' critique of Gadamer's *Warheit und Methode* (Mohr, 1965), in his *Zur Logik der Sozialwissenchaften* (Suhrkamp, 1970) is likely to be of greater contemporary interest than Thomas' criticisms of an earlier generation of anti-naturalists. (An English translation of Habermas' critique can be found in *Understanding and Social Inquiry,* eds. Fred R. Dallmayr and Thomas A. McCarthy [Notre Dame, 1977, pp. 355-63].) But alas, Habermas is mentioned only in passing, and Gadamer not at all. (Is Britain, philosophically speaking, still an island fortress?) Still, despite its several shortcomings, *Naturalism and Social Science* repays a close and careful reading.

TERENCE BALL

University of Minnesota

John Locke: Economist and Social Scientist. By Karen Iversen Vaughn. (Chicago: University of Chicago Press, 1980). Pp. xiv + 178. $14.50.)

Despite the wealth of material published in recent years on the thought of John Locke, there has been no systematic attempt to analyze his formal economic theory and its relationship to his political thought. Indeed, many political theorists seem to be unaware that the champion of early liberal ideology was also a major contributor to the economic debates of the seventeenth century, and an important figure in the history of modern economic analysis. In *John Locke: Economist and Social Scientist,* Karen Vaughn attempts to redress this imbalance in current Lockean scholarship by providing a new perspective for understanding the nature of Lockean economic theory and its connection to Lockean political philosophy.

According to Vaughn, the foundation to Locke's economic analysis was his theory of value. "It was his tool, his one model for dealing with all economic problems" (p. 17). Significantly, this theory of value was not the famous labor theory of value that is found in *The Second Treatise,* nor was it an early version of the supply-and-demand analysis that is the basis of modern micro-economics. On the contrary, it was, as Vaughn explains, a theory of value which was creatively derived form a comparison of the proportions between the quantities and the vents of goods bought and sold on the open market, and relied heavily on both Aristotelian doctrine and current scientific language for its formation.

The significance of this theory of value to Locke's thought was twofold, argues Vaughn. First, it provided him with an explanation of the price of commodities and a theory of money, of interest, of rent, and of foreign exchange. Second, and from Vaughn's perspective just as importantly, it provided him with a set of economic principles for objectively understanding the operations of a market economy. Indeed, Vaughn concludes that "by adopting a disinterested scientific approach to his subject," Locke was able "to arrive at a set of principles describing the behavior of essential economic variables and thereby to provide men with more reliable knowledge of how to increase their national wealth" (p. 136).

Following the inquiry into Locke's theory of value, much of Vaughn's study centers around an attempt to show how the principles found in Locke's economic writings complemented those found in his political writings and ultimately reflected a "comprehensive theory of society" which was based on an understanding of human nature and which presumed some natural order. For example, in a fasscinating account of Locke's theories of interest, of rent, and of foreign exchange, Vaughn explains how Locke's political belief in limited government neatly dovetailed with his economic belief that the government could not arbitrarily intervene in the economy without facing severe unintended consequences. This general line of argument is pursued further in her discussion of the *Two Treatises*. Vaughn goes as far as to argue that "Locke's theory of the origin and purpose of government rested in large part on his understanding of the nature and consequences of economic activity" (p. 111).

An unsatisfactory aspect to Vaughn's book is her attempt to establish the fact that John Locke was a "social scientist." While she provides a rather straightforward definition of social science as "an attempt to explain human social interaction in a systematic way, based on assumptions about the characteristics of individual human beings" (p. xii), she makes little attempt to link up the "social science" of Locke's economic and political writings to the theory of knowledge or the theory of human behavior found in Locke's *Essay Concerning Human Understanding*. Without a serious consideration of the *Essay*, any discussion of Locke as a social scientist is necessarily incomplete. This problem aside, Vaughn's book is an important and useful addition to the growing body of Lockean scholarship.

EDWARD J. HARPHAM

University of Houston

Margins for Survival: Overcoming Political Limits in Steering Technology. By Edward Wenk, Jr. (New York: Pergamon Press, 1979. Pp. xiii + 199. $26.00, cloth; $10.50, paper.)

In the past several decades developed and developing countries have been increasingly transformed into "technological societies" (Jacques Ellul) or "post-industrial societies" (Daniel Bell). Technology has become a virtual elixir for solving the problems of war, crime, poverty, energy, nutrition, and transportation, among others. New technological developments have been used by national governments both as engines of social change and for social control. Increasingly, however, observers are growing concerned about the negative influences of technology on society.

While Edward Wenk does not accept the most negative interpretations of technological development, neither is he overly optimistic. *Margins for Survival* is a personal essay on the study of the future—a future that may never occur because the current policy apparatus seems to be more attuned to short-run concerns. The book can be divided into two parts. The first contains an inventory of threats to survival ranging from nuclear terror to the loss of freedom, as the subtitle to chapter 2 indicates. Most of these threats involve a major technology that, if developed, would yield both benefits and adverse consequences. Here Wenk turns the analysis on its head. Whereas most writers treat technology as an independent variable, studying its impact on society, Wenk is more interested in the impact of society on technological development. It is his thesis that "political limits" are more important to this process than market signals or mechanisms. Government defines "the relative responsibilities of public and private sectors, the selection of benefits and beneficiaries, and the recruitment of public resources" (p. 57). However, one of the failures of current policy making, he argues, is the failure to incorporate signals about the future into current decisions. This is because of the fact that "the swift pace of technological change no longer matches the response time of human affairs" (p. 4). The result, says Wenk, is a series of "pathologies of the short run" which prevent us from looking ahead and anticipating the second-order consequences of our decisions.

The second part of the book, comprising chapters 6 through 11, contains a prescription for institutionalizing the future in the decision-making process. The author begins this section with a list of prescriptions for studying the future which can be found in a broad cross section of the extant literature. He couples these with his own remedy —a transformation of the cultural setting in which decisions about the future are made. Although constrained by scarcities of time, personnel, and reserves, Wenk's prescription includes the incorporation of future studies in educational curricula, a new collaboration among scientists, engineers, and citizens, the enhancement of technology assessment capabilities, the integration of anticipatory analytical capabilities across government institutions, and the creation of conflict-resolving institutions.

This book makes an important contribution to the study of decision making for the future because it makes explicit the interrelationship between technology and the political process in the form of a "technology delivery system." Wenk skillfully integrates a broad range of literature on science, technology, the political process, and the future. This book should be particularly useful for policy makers because it highlights the political problems in adopting what might seem to be rather facile technological solutions to pressing national problems. However, the book is limited in its usefulness. It is primarily a personal and prescriptive statement about decision making for the future, and is so intended by the author. The absence of an analytical framework is obvious. The proffered prescriptions are too briefly sketched to be applied, and it is not always clear why they should be preferred over the present situation. The reader would be shortsighted indeed who did not examine these prescriptions with the same critical eye which the author turns on the present policy system.

ALLYN R. BROSZ

Virginia Polytechnic Institute and State University

Biopolitics: Search for a More Human Political Science. By Thomas C. Wiegele. (Boulder, Colo.: Westview Press, 1980. Pp. xiv + 159. $17.50, cloth; $8.00, paper.)

Over the last decade, an increasing number of scholars have sought to relate the biological sciences to the study of politics. Often described as "biopolitics," this trend has been paralleled in such fields as economics, sociology, anthropology, and even psychiatry.

Ten years ago, links between human behavior and biology were primarily literary: the popularizations of Konrad Lorenz (*On Aggression,* Harcourt, 1966), Robert Ardrey (*African Genesis,* Atheneum, 1961, and *The Territorial Imperative,* Dell, 1968), and Desmond Morris (*The Naked Ape,* McGraw, 1967). Today, these pioneer efforts are frequently criticized for superficiality or imprecision; scholarly research in biopolitics is slowly becoming legitimate within political science itself.

Thomas Wiegele is among those who have been active in this trend, having established the Center for Biopolitical Research at Northern Illinois University. He is therefore well placed to provide a progress report on the emerging subfield.

Biopolitics: Search for a More Human Political Science deals with an area of research rather than a specific hypothesis, theory, or problem. It does not pretend to synthesize many disciplines as they relate to a single phenomenon (like Lionel Tiger's *Optimism: The Biology of Hope,* Simon, 1979). Rather, Wiegele limits his survey to recent works by political scientists, showing how our discipline can be enriched by a biological point of view.

Critics of such an approach have frequently condemned it as "reductionist" or "determinist." Wiegele's *Biopolitics* provides both an argument, and some evidence, against this conventional misinterpretation. The argument, summarized by the book's subtitle, is that "political analysts have failed to take into account the realities of political life, *at the level at which it is lived.* At this level, biological considerations interact with emotional, rational, and psychological factors within a real human being" (p. 84, italics in original). Scholars tempted to disagree would do well to reflect on how well they think with a splitting headache.

The sheer diversity of topics covered by recent "biopolitical" research is further evidence that the field is hardly reductionist or determinist. As Wiegele puts it, "Political beings are not ethereal essences floating around in an intellectual construct called a political system. Because human nature is rational, psychological, *and* physical, analysts of political behavior must begin to conceive of the subjects of their investigations as biological beings with intellective capacities. . . . Thus it is incumbent upon us to add a biological perspective to our work; in doing so we need not engage in a blind acceptance of a biological determinism that pays no heed to human purposive activities" (p. 145).

For example, Wiegele's own research (summarized on pp. 92-95 and 116-19) shows how new approaches inspired by the life sciences can enrich traditional political science. By treating physiological stress as a vital—and measurable—factor in the behavior of political leaders, our understanding of both domestic politics and international crisis is greatly increased.

The reader may be frustrated by the lack of depth in the treatment of each of the many issues touched upon in this slim volume. But to gain a thorough background in biopolitical research, by definition, one must study specific areas in an interdisciplinary perspective. And here, Wiegele's intention is to survey *only* the work of political scientists, rather than to follow any one line of research to its completion.

Because *Biopolitics* considers only recent work, it does not stress the inclusion of biological factors in political theory from Aristotle to Rousseau, Marx and Kropotkin. As with any survey, one can complain about the omission of some authors (Henry Beck, Elliott White, Claude Phillips, and William Ophuls come to mind). Perhaps more important, Wiegele fails to indicate that the parallels between inclusive fitness theory in biology and collective goods or public choice theory, already noted by economists like Gordon Tullock and Jack Hirschleifer, cannot fail to have an effect in political science.

But such caveats only strengthen the main point of *Biopolitics*. As Wiegele says in his conclusion, "It is no longer a question of *whether* we should incorporate human life science data into our studies of human social behavior; that is already being done by the life scientists. . . . The more crucial questions are: who is best equipped to study human social behavior, the life scientist or the social scientist, and who will have a deeper appreciation of humanist values, the life scientist or the social scientist?" (p. 155).

ROGER D. MASTERS

Dartmouth College

Power: Its Forms, Bases and Uses. By Dennis H. Wrong. (New York: Harper and Row, 1979. Pp. viii + 236. $28.50, cloth; $6.95, paper.)

When asked how he distinguished a mathematician from one merely proficient at mathematics, Norbert Weiner replied, "By the problems he chooses to work on." The same standard can be applied to social scientists concerned with political analysis, although the results are usually discouraging, especially lately. Nonetheless, by this and several other basic criteria, Dennis Wrong's study of power comes off well. It is an important work on a most important topic—perhaps the core concern of political science addressed by a well-known sociologist.

The book is accurately titled. It deals primarily with the forms (types), bases (resources), and uses (social significance) of power. It is not and does not purport to be a comprehensive discussion of all the major topics of power analysis. Largely excluded from systematic scrutiny are such central matters as the epistemo-scientific status of the concept of power, various key aspects of power relations, many features of the psychology of power (especially cognition), the measurement of power and power structures. Useful comments are made regarding some of these topics, but they are less developed than the dimensions designated in the title.

With some fanfare, Wrong builds intention into his conception of power, defining it as "the capacity of some persons to produce intended and foreseen effects on others" (p. 2). However, power is not for him the most general conception of interactor causation. He allows a residual set of "unintended influence" relations which, together with power, make up the total realm of "influence." Thus some analysts may feel that merely labeling preferences are involved. However, Wrong insists both early and late in his book that his "influence" (others' "power") is tantamount to any social interaction and therefore too general a topic to pursue fruitfully. He sees such a notion as equivalent to the subject matter of all social science. On the contrary, however, one might either demur to the charge or contend that power (Wrong's "influence") is merely one aspect of all social interaction—focusing on its causal features. One can also focus, for example, upon its economic, communication, prestige, or emotional features. In general, the question of the likely boundaries of the phenomenal realm related to power for which a relatively coherent body of concepts and theories can apparently be developed demands a more extensive and penetrating analysis than Wrong attempts. Fortunately, most of Wrong's other comments apply very well to partly intended, non-intended and anti-intended power relations.

Wrong categorizes power into four types: force, manipulation, persuasion and authority. Authority is very broadly conceived, à la Easton, as "successful ordering or forbidding" (command-obedience), regardless of legitimacy (p. 35). It is then further categorized into coercive, induced, legitimate, competent, and personal types, the last two essentially being French's and Raven's "expert" and "referent" power, respectively. In the process, Wrong makes a number of judicious comments not only about these various types of power and authority, but also about the recalcitrant problems of fit between analytic distinctions and concrete reality. Consequently, though one may find portions of this particular typology uncongenial (e.g., the confusion of hybrid types such as manipulation and authority with more fundamental types such as force or reward), one may still profit significantly from the discussion.

An outstanding feature of the book is Wrong's analysis of what he calls the "combinations and interrelations" of the forms or types of power—i.e., "regular 'tendential laws' of transition from one form to another" (p. 66). For example, he hypothesizes, with illustrations, that regular persuasion is likely to become personal or competent authority, that regular reward is likely to become seen as threat of punishment, that continuing coercion tends to become legitimized by both influencer and influencee, and that there is an ineluctable interplay between force and the threat of force. Like most of the book, this discussion is very general and essentially speculative, paying meager attention to empirical confirmation or intermediate elaboration; but it is also sophisticated, stimulating and sensible.

After the forms (types) of power, the next largest focus of attention is resources—a topic very feebly treated in the literature, with Baldwin's work (not noted) the main exception. Like his predecessors, Wrong unhappily makes no attempt to define or explicate the elusive notion of a power resource. (Doing so may reveal many resource-invoking political hypotheses as patently tautological.) He concentrates on "liquidity" ("fungibility"), on the problem of "objective interests" (well handled), and, particularly effectively, on collective as opposed to individual resources. Solidarity and organization are seen as the two most crucial collective resources and furnish the analytic basis for an interesting perspective on political mobilization.

Rather naturally, the very range and difficulty of the discussion exposes flaws. The two-edged nature of "anticipated reactions" (democratic and anti-democratic) is aptly noted, but too much is made of difficulties for power analysis posed by anticipated reactions (no reference is made to March's telling analysis). The discussion of power motivation neglects most of the psychological writing on the topic (e.g., Winter and McClelland). The treatment of the "uses" of power deals mainly with other matters or else handles such concerns as power structures or the zero-sum problem less than profoundly. The discussion strives to be multi-disciplinary but fails to do justice to the contributions to power analysis of either political science or social psychology. On the other hand, Wrong performs a very useful service for many readers by calling American attention to many of the British and Commonwealth writers on power who are frequently ignored.

The overall orientation of the book, understandably, is heavily sociological. Marx, Weber, Dahrendorf, Parsons, Mills and company are constant points of reference, sometimes helpfully, as with Marx and Weber, sometimes rather fruitlessly, as with Parsons, who was not a very effective power analyst. But if one allows for the limitations of different disciplinary perspectives, one can gain from them rich insights not forthcoming through familiar eyes. Wrong has given us a thoughtful, liberal and provocative analysis of many key features of an utterly vital phenomenon. Since Lasswell's and Kaplan's *Power and Society* (Yale, 1950), few works on power of its range and depth have appeared in any discipline. One may wish to argue with the author about many of his particular positions, but one cannot help but profit from exposure to them.

FREDERICK W. FREY

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American Politics

Friend and Foe in the U.S. Senate. By Ross K. Baker. (New York: Free Press, 1980. Pp. xiii + 301. $12.50.)

Ross Baker, former Senate staffer and currently a professor at Rutgers, has written a book on friendship and enmity in the U.S. Senate. Much of the book is devoted to the types of "friendship" he finds, based on interviews with 25 past and present senators and secondary sources dealing with a wide span of Senate history. The most common form of friendship found is that of "institutional kinship," which involves the expectations of pairs of senators that their colleagues will be restrained, diligent, empathetic, and trustworthy. Baker suggests that relationships between individual senators, based on these expectations, are bonding agents that allow the Senate to function, despite somewhat weakened general norms. Baker describes other forms of friendships between pairs of senators including "political alliance," the least intimate and personal form of friendship except for institutional kinship, "social friendship," both intramural and extramural (occurring within or also outside of the Senate, respectively), and "pure friendship," which is closest to what we most often mean by the term friendship in our own daily lives. As one moves along the typology from institutional kinship to pure friendship, we find, not surprisingly, fewer cases.

Baker examines the factors that seem to contribute to the friendships found in a Senate he sees as competitive and individualistic. Not surprisingly, shared political values are important in personal and political friendships; however, they do *not* assure friendship. For example, Baker points out the strong conflicts that sometimes occur between senators of the same state and party, as with Hatfield and Packwood of Oregon, and also shows that political and ideological compatibles are not always close. He also shows how common social ties, and especially religion, can facilitate friendship. In fact, the strongest contemporary pure friendship case he finds involves four senators who are "Evangelical Christians," Nunn, Chiles, Bartlett, and Domenici. Unfortunately, a substantial part of the author's information on these pure friendships—and, for that matter, on feuds between senators—is based on his speculation combined with secondary sources whose validity we cannot readily assess. Baker also looks at the political significance of the types of friendships he sees. He finds the Senate's institutional cohesion facilitated, especially by institutional kinship, political alliance, and intramural friendships. He suggests how intramural friendships can provide, to use Hubert Humphrey's phrase, "wiggle room." He presents one contemporary case where pure friendship actually resulted in successful persuasion, senator to senator. However, Baker is careful to point out that senators are not likely to hurt themselves or their state for a friend and are not expected to; he also suggests they shouldn't. From a political perspective, Baker argues, deeper friendships between senators have diminishing marginal returns, which many senators appear to realize. The growing politicization of a senator's social life, as evidenced in fund raisers and politically motivated receptions, also seems to limit opportunities and incentives for deeper friendships within the body as does the Senate work load and rivalry between colleagues. This is evident in the judgments of House alumni whom Baker interviewed, who found it harder to form close friendships in the "upper body" than in the House.

Baker's chapter on senatorial strife is interesting. He lists the personal traits senators most dislike (there were no surprises) and the potential sources of feuds, including jurisdictional conflict, bill or issue thievery, intervention in a colleague's election, efforts to gain the presidency, and leadership fights. Most important, however, Baker finds a lack of consistent overt gestures of malice among the feuding and suggests they are too costly and not worth the trouble. Rather, feuds are more likely to lead to a disinclination to accommodate where a senator would not be hurting himself, his state, or his positions by so doing. Baker's chapter on lonership and leadership shows how some personal, though not political loners (Borah, Taft, Knowland, and Byrd) have been selected as leaders and speculates why this occurs, but his discussion of what senators want in their leaders contains little that is new.

Baker's book is well written and will be of some interest to the devotee of Congress, but it contains little that is important *and* new. Though this is a book on friend and foe in the Senate, the humanness of that institution is not adequately captured, perhaps because senators are somewhat private in what they're willing to share and how they do so. The book is summarized quite nicely and usefully in the last chapter. However, most of what is significant in the work is already known by congressional scholars. The book reads well and is enjoyable in places. Regrettably, that can be said of too few other works we read.

SIDNEY WALDMAN

Haverford College

The Pulse of Politics: Electing Presidents in the Media Age. By James David Barber. (New York: W.W. Norton, 1980. Pp. 342. $14.95.)

By the time this review was prepared, *The Pulse of Politics* had already been promoted by announcements on National Public Radio, discussed by its author on such television shows as *Good Morning America,* and commented upon in both Sunday supplements and journalism reviews. And by the time this review appears a variety of scholarly judgments will already have been pronounced. Hence a descriptive rehash of Barber's thesis may be unnecessary. Put briefly, it is that the pulse of presidential campaign politics beats in a 12-year rhythm, distinctive reverberations sounding each 4 years during the presidential campaigns. First pounds forth the politics of conflict —i.e., the presidential campaign is a battle for power (1900, 1912, 1924, 1936, 1948, 1960, 1972). But the nation tires of battle and seeks redemption; hence the next pulsation is the politics of conscience (1904, 1916, 1928, 1940, 1952, 1964, 1976). But the national spirit is not soothed, cleavages do not vanish, selfish interests do not recede. So four years later appeals for harmony and happiness echo through the campaign, the politics of conciliation (1908, 1920, 1932, 1944, 1956, 1968, 1980). Conflict, conscience, conciliation—and as Sonny and Cher once sang, "The Beat Goes On." That pulse is most readily detected in the news media. The journalist, writes Barber, is a storyteller. And every four years the news media sound a dramatic theme for each presidential campaign —first the war story, then the morality play, finally the romantic comedy.

Cyclical theories of politics have had their critics since at least the time of Polybius. Detractors note that cyclical theorists occasionally and unintentionally interpret events as fitting one cycle period when it would be just as easy to identify those events with a different period of the cycle. *The Pulse of Politics* has this tendency. Granted that Barber marshals considerable historical evidence to justify, say, labeling the 1912 campaign as conflictual, or 1928 as the politics of conscience, or 1956 as conciliation. Sometimes, however, the proof lies in redefining the personae of the campaigns or the settings of the campaign. Thus the 1900 campaign pitted not the "sweet-tempered" incumbent, William McKinley, against the Democrat's William Jennings Bryan, but McKinley's stand-in, Vice-President Theodore Roosevelt, against the silver-tongued orator. The combative Roosevelt set the tone for a politics of conflict, at least in news accounts. Apparently Teddy Roosevelt set the same tone in 1912, for the ex-professor (and four years later the moralist) Woodrow Wilson was eclipsed in the struggle of Taft versus Teddy.

But such matters of deciding just who were the principal candidates in specific campaigns are, as sports talk goes, "judgment calls." Depending upon the journalistic accounts examined and the historians making the calls, judgments will certainly vary. A greater problem emerges in redefining the setting of the campaign drama. In his critique of Barber's book, published in the *Columbia Journalism Review* (19: 78-79), Ralph Whitehead, Jr. noted only one "obvious clinker" in the 20 campaigns classified in *The Pulse of Politics.* Whitehead thinks that to be 1972, but does not say why. There is a reason and that reason calls into question the typing of the campaigns of 1968, 1976, and 1980 as well.

Barber notes that in 1972 Richard Nixon did little campaigning, opting instead to remain above the fray and letting surrogates speak for his record. With neither president nor surrogates in a combative mood, where was the politics of conflict to emerge, especially when the Democratic nominee was a highly moralistic "deacon from Dakota" (conscience) who sounded in his acceptance speech a plea for "reconciliation" with "Come Home, America" (conciliation)? As it turns out, the "story of politics as war" was dramatized not in the general election, but in the Democratic primaries. If this be so, however, can one easily dismiss the war for the 1968 Democratic nomination, for control of that party's nominating convention, and for control of that party during the general election? Were the news media really spinning yarns of conciliation when many readers/viewers thought it was conflict? And, what of 1976? News stories of caucus fights, primary battles, and struggles for control of the convention (first about Democrats, then Republicans) were surely as much The Big Story of 1976 as post-nomination themes of a "government as good as its people" or "he made us proud again."

Critics of cyclical theories point to another problem—i.e., a number of different cycles can be discovered to cover the same events. Different assumptions yield different cycles. In many respects *The Pulse of Politics* is to America's collective consciousness what *The Presidential Character* was to the consciousness of individual politicians. Freud's ego, superego, and id now meet Jung's archetypes. But there are other cyclical bases. What if some religious mythology is correct, for example, and we are bound by an endless cycle of sin, suffering, sacrifice, and salvation; how does that cycle fit the politics of 1964, 1968, 1972, and 1976, or each of the 16-year cycles preceding? Or consider Peter Kaplan's semi-tongue-in-cheek version of America's political cycles (*Esquire,* April, 1980, pp. 41-47). He suggests that we may alternate "soft" and "hard" eras of undetermined length. The late 1960s and the 1970s were "soft-line," i.e., vulnerability was the code word, self-indulgence and self-involvement the styles. The final act in the soft-line drama, thinks Kaplan, came in July, 1979, when Jimmy Carter asked Americans, "Whenever you have a chance, say something good about our country." But then came hostages in Iran, Soviets in Afghanistan, inflation, recession. Toughness, belt-tightening, and team play—the hard line—was in vogue again. A new, hard-line Carter and a hard-line Reagan contested for the presidency in 1980.

Cyclical theories, then, have their problems. In the face of them, Barber must be admired for advancing a new cyclical theory of presidential politics. It is imaginative and in many instances persuasive. There are ways, however, in which the overall case might have been even more persuasive. One wishes, for example, that Barber might have addressed directly how his vision of American political events contrasts with those of key historians such as those expressed by Richard Hofstadter in various volumes. And students of voting behavior might wonder how Barber's classification of presidential elections matches with those of *The American Voter,* V. O. Key, Jr., Gerald Pomper, or Charles Sellers. How do voters respond to what Barber calls "the interplay of Presidential campaigners and the journalists who tell their running stories?" If the cycle of conflict, conscience, and conciliation does not, as Barber says, "point out the winner," how does the cycle interact with factors which voting studies say influence electoral outcomes? Moreover, is what candidates actually do, journalists' reports, or both the key to making an election one of conflict, conscience, or conciliation? John Carey has noted (*Journal of Communication* 26: 50-57) that journalists write more about the meta-campaign than the campaign. Which stories define the pulse of the election? Finally, just as the pulse metaphor runs throughout the book, so does the dramatic motif. Yet there is a sense that dramatistic analysis in the tradition of communication scholars (Kenneth Burke, Hugh Duncan), sociologists (Georg Simmel, Peter Berger, Erving Goffman), or political scientists (Murray Edelman, Richard Merelman, James Combs) was deemed unrelated to the drama of electing presidents in the media age. That was unfortunate.

Barber believes that the cycle that is the pulse of politics can be broken and that it is a key responsibility of journalists to do it. Not only must all three stories—conflict, conscience, and conciliation—be told in each election, but also each must have greater depth: underlying facts yielding conflicting visions should be revealed, the characters of candidates seeking to guide the nation's conscience should be explored, and the nature of the coalitions flowing from conciliations should be analyzed. Should the cycle be broken, it is hard to say whether life would be softer or harder for presidential candidates. Given the current political environment (July, 1980), perhaps things are already out of synchronization. At least the active-positive incumbent might well wonder if 1980 is a campaign of conciliation.

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Judicial Review and the National Political Process: A Functional Reconsideration of the Role of the Supreme Court. By Jesse H. Choper. (Chicago: University of Chicago Press, 1980. Pp. xviii + 494. $28.50.)

Jesse Choper has blended democratic theory, constitutional history, and political science in this brilliant "functional reconsideration" of the Supreme Court. Choper's initial chapter addresses the conundrum that has vexed constitutional scholars—reconciling the place of the Court in a democratic society. Not since Leonard Levy's essay of over a decade ago have the various tensions inherent in (and possible justifications for) the Court's role in a majoritarian political system been examined so elegantly. Moreover, drawing upon the wealth of modern political science analyses of Congress and the presidency, Choper reviews the undemocratic aspects of the other branches. He concludes that, although the other branches are slightly bent in undemocratic directions, there is a central, undeniable, and upright core of popular responsibility. The Court is neither as democratic nor subject to popular restraint.

Given the Court's undemocratic nature, Choper seeks to define its proper role. But rather than deal with the substantive results of the Court's decisions. Choper attempts to prescribe a standard of justiciability—to distinguish judicial circumstances requiring the Court's involvement from those in which the Court's judgment is unnecessary or counterproductive. Choper's prescription, in the form of four "proposals," is the result of a complex consideration of the realities of the national political process, the reversibility of the Court's action, and the likely drain upon the finite resources the Court possesses for enforcing its own decrees.

Thus, in his "Individual Rights Proposal," Choper argues that the overriding justification for the exercise of judicial review is the protection of individual liberties. The essential rationale for this position is well known—such a role for the judiciary is contemplated by history, reinforced by modern judicial opinion, and functionally justified to protect the unpopular and unrepresented minorities in a majoritarian system.

This is not the case for questions involving national infringements upon state powers, however. As Choper shows, the states are well represented in the national political arena and have many opportunities in the legislative process to impose their vetoes. Moreover, on those occasions when the Court has issued "states' rights" decisions, it has been the victim of what Hughes called "self-inflicted wounds" to its public standing. Thus Choper's "Federalism Proposal" advises that such conflicts be held nonjusticiable and left for political resolution. On the other hand, Choper does envision federal judicial oversight of state actions that might threaten the exercise of constitutionally delegated national powers. Not only does the lack of national representation in the state legislatures justify this role, but also the Court's actions are easily reversed by a Congress dissatisfied with the justices' defense of its powers.

Choper's "Separation Proposal" requires the Court to abstain from most of the battles between Congress and the president. Each branch has jealously guarded its powers and possesses the weapons necessary to preserve its prerogatives. Not only is the Court's involvement in such inter-branch disputes unnecessary, but should either branch flagrantly exceed the powers assigned to it, "there is scant reason to believe that the Court will succeed in halting the march" (p. 305).

Nevertheless, Choper does reserve for the Court the power to review attempted restrictions or enlargements of the judicial power by the other branches. Choper premises this "Judicial

Proposal" upon the "distinctive ability of the Justices" to define their power (p. 384), the Court's inability to protect its jurisdictional boundaries in the national political process, and the experience of two centuries.

The only weakness of the book rests upon a well-worn path. Although the rationale for the Court's protection of the rights of the minority is well understood, defining the minorities and the specific rights warranting protection is a "demanding and delicate" task (p. 64) that Choper only recognizes as "awesomely perplexing" but does not undertake (p. 77). Nor does he satisfactorily address the criticism that though the Court is in the best position to protect individual liberty, there is no guarantee it will assume that posture. Although Choper clearly expects the Court's substantive decisions to be made "in behalf of personal constitutional liberties," he admittedly offers no advice to the Court on "how this superintendence should be carried out" nor any guarantees to the rest of us that it will be carried out at all (p. 79).

Otherwise, the book's strengths are undeniable. Choper carves careful exceptions in each proposal to allow the Court to exercise its involvement when questions of individual liberty overlap with but stand independently of questions of federalism, separation of powers, or judicial power. And each proposal is examined rigorously and subjected to equally stringent self-criticism, tested in a variety of past and hypothetical constitutional scenarios. With his review of the modern Court's defense of individual rights against state infringement, Choper for the first time musters empirical evidence to counter the criticism that the Court has rarely served as the protector of individual liberty. And Choper's subsequent review of noncompliance with the Court's rulings suggests that the Court expends large sums of what he calls its "exhaustible institutional capital" to resolve disputes in which its intervention is unnecessary and which at the same time threaten its enforcement of individual rights decisions.

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Constitutional Government in America. Edited by Ronald K. L. Collins. (Durham, N.C.: Carolina Academic Press, 1980. Pp. xiii + 504. $24.95.)

This hefty volume consists of essays and commentaries presented at Southwestern University Law Review's first west-coast conference on constitutional law, held in September, 1977. The purpose was to commemorate the 190th anniversary of the writing of the federal Constitution. The 36 contributors include 16 law professors, 6 lawyers, 5 judges, 3 history professors, 2 professors of political science, a public official, a magazine editor, and 2 professors of things in general. Thus the views expressed in this symposium are, for the most part, those of professional legal scholars.

The volume deals with a very wide variety of subjects. Six essays are devoted to First Amendment problems, 2 with rights of privacy, 3 with criminal justice, 2 with due process, 8 with equal protection questions, 3 with access to the federal courts, 3 with federalism, one with land use, 3 with some aspects of constitutional history, and 3 constitute a brief debate on judicial review. Speaking generally, as one of the authors suggests, constitutional law has not been a very important subject until fairly recent times, and the growing attention devoted to constitutional law analysis is evidence of the ever-heightened stature of constitutional interpretation in our governmental system. The heavy emphasis on the concept of the equal protection of the law suggests its centrality in current American constitutional law. Even so, it is a fair guess that this too will pass, and that at some future time, when the essential facts of life have changed, some other legal doctrine will take center stage. For if there is any single theme in this diverse collection of essays, it is that fundamentally the Constitution has survived and served us well as the jumping-off point for so much complex and lively debate because its brevity and the generality of so many of its key provisions have supplied the element of flexibility which is the essential predicate of constitutional interpretation. However one may choose to assess various positions advanced in this book, no one can possibly miss the point that these scholars are talking about a living constitution and that constitutional interpretation is something more than an exercise in antiquarianism.

A volume of conference papers delivered by so many diverse people is bound to be something of a potpourri. Some essays are very substantial and fully annotated; others are little more than brief informal comments. Furthermore, while this purports to be a collection of essays on constitutional law, some are not really in this category except in a very loose sense, such as Donald McDonald's essay on the press or Alan F. Westin's essay on privacy. In addition, while major topics of constitutional law are ignored or touched upon only tangentially, such as the commerce power, the taxing power, interstate relations, the powers of the president, and the control of foreign relations, there is an essay on due process limitations on the impeachment power and one on the constitutional limits on land use control (zoning). Incidentally, the land-use essay, by R. Marlin Smith, is very well done, and well worth reading.

It is to be expected that in a large miscellany of this sort, one runs into observations which lead to eyebrow-raising, at least on my part. Thus, William M. Kunstler asserts that "the search and seizure protections of the fourth amendment . . . have been virtually destroyed" (p. 305). This strikes me as a very considerable exaggeration. To be sure, the Burger Court has trimmed down on the exclusionary rule, but this hardly warrants Kunstler's gloomy conclusion. In this connection, attention should be drawn to the essay on the exclusionary rule by Judge James L. Oakes, of the U.S. Court of Appeals for the Second Circuit, which presents a favorable analysis of this much-debated topic. To cite another example, Carole E. Goldberg-Ambrose asserts that "the Supreme Court has taken special pains to protect federal jurisdiction from congressional enlargement" (p. 321). This struck me as a dubious observation; if indeed the Court has taken such pains to prevent Congress from enlarging federal jurisdiction, it has certainly failed in its objective. Another example is a remark made by John P. Frank that Justice Blackmun is "very much a heavy-weight in the present scheme of things" (p. 478). With all due respect to John Frank, who is one of our leading and most learned Court-watchers, and to Justice Blackmun, who for all I know is a fine man, it never crossed my mind to describe him as an intellectual heavyweight. On the other hand, if Frank means that he is a heavyweight when compared to his current colleagues, then I must say that this is a perfectly devastating description of the quality of the sitting justices. Finally, it is inevitable, I suppose, that in a book of this sort, a great deal of effort is devoted to proving things that no longer require proof. For example, taking as his starting point the deaths of Justices Murphy and Rutledge in 1949, Irving Dilliard seeks to demonstrate that changes in the personnel of the Court have an effect on the course of its decisions (pp. 447-56). I suspect that at this late date this is a self-evident proposition which requires no further proof.

The first six papers in this book are concerned with various aspects of First Amendment freedoms. The best of the lot is a learned, well-argued essay by Steven Shiffrin on defamatory non-media speech. On the other hand, the equally exhaustive and fully annotated essay by C. Edwin Baker on the scope of First Amendment freedoms raises some questions in my mind. His demolition of the classic marketplace-of-ideas theory is worthy of the attention of all constitutional law scholars, but his brief analysis of religious freedom strikes what seems to me a very dubious note. Starting with the assertion that the Court wrongly decided the polygamy case, *Reynolds* v. *United States,* 98 U.S. 145 (1879), Baker then goes on to take issue with the famous remark of Justice Roberts in *Cantwell* v. *Connecticut,* 310 U.S. 296 (1940), to the effect that freedom to believe is absolute, but not the freedom to act. Then Baker asserts: "To deny conduct any protection clearly would gut the meaning of '*free exercise*' " (p. 82). A short answer to this sort of card-stacking is to point out that Roberts did not say that *no* conduct under the guise of religion is entitled to any protection. On the contrary, he said that "conduct remains subject to regulation for the protection of society." I have always assumed this to mean that where the protection of society requires it, conduct may be regulated. This seems to be a truism, that religious freedom ends where the criminal code begins. Baker says, on this point, that this is no solution "unless one can establish definite limits to the scope of criminal law" (p. 84). A short answer is that the criminal law goes as far as it goes. I must confess that I do not understand what Baker thinks he is proving with respect to religious freedom. If he means that anything goes so long as a religious tag is attached to it, then I must say that I dissent.

In addition to First Amendment freedoms, the topics that receive the heaviest emphasis are those related to federalism, the equal protection of the laws, and judicial review. In respect to federalism, Martin Shapiro says: "Within the intellectual framework of sovereignty, federalism is an absurdity, a contradiction in terms" (p. 360). That is to say, it is logically impossible to have two sovereignties involving the same people and the same territory. Of course this is true, but American federalism need not necessarily be defined in terms of two sovereignties. What makes our system federal is that the powers of the states are not mere delegations of authority by the national Congress. They are residual powers *constitutionally* reserved to the states. Without saying so in so many words, Shapiro seems to suggest that the states are largely administrative agencies of the national government, and of course however federalism is defined, it is not definable as merely a system of administrative decentralization. But the reader may take heart, for, if one keeps on reading, one will take note of Judge Charles E. Wyzanski's observation that "the greatest contribution politically made by our country has been in its preservation of federalism" (p. 490).

Most aspects of equal protection which are currently before the country are touched upon in these essays, particularly the vexing problems of affirmative action, and discrimination on the basis of sex and wealth. The brief remarks at the end of the book on judicial review add little to what has long been common knowledge on the subject.

Finally, I should like to call special attention to several absolutely first-rate essays. One is George Anastaplo's brillliant dissection of Abraham Lincolin's Emancipation Proclamation. Another is Scott H. Bice's fine essay on the law of standing, which is at present a lively subject of controversy. Still another well worth reading is the essay on administrative due process by Bernard Schwartz. Of course, several other essays in this book are works of solid scholarship. On the whole, however one may choose to cavil here and there, this is a book which will find a solid and enduring place for itself in the literature of American constitutional law.

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Energy Policy and Public Administration. Edited by Gregory A. Daneke and George K. Lagassa. (Lexington, Mass.: Lexington Books, 1980. Pp. vii + 323. $29.95.)

This book well demonstrates the pitfalls of editing a multi-authored volume. Such volumes often lack an overall coherence and reflect a situation in which individual authors (and sometimes the editors themselves) are only remotely familiar with what other contributors have written. *Energy Policy and Public Administration* is no exception; indeed, it seems to be a classic example.

One of the book's major purposes is to discuss the variables affecting the nation's energy use and to "specify realistic strategies for achieving a balanced future" (p. vii). For many of the contributors this means an advocacy of soft-path strategies emphasizing renewable energy resources at the expense of high technologies using nonrenewable resources.

Advocacy has its place, and prescription is surely in the realm of policy analysis. To be effective, however, recommendations for change ought to be consistent and certainly not contradictory. Inconsistency and contradiction serve only to confuse policy makers and to diminish the prospects for change along desired lines. The present book provides many illustrations of the latter situation. For example, one key question concerns the government's appropriate role in the energy arena. At least two authors, Walter Mead and Marc Ross, favor a market approach with very limited government intrusion. Several other authors urge much greater government involvement and many more government programs. Daneke rails against the institutional barriers that the U.S. government has created. As he puts it, the path toward the use of renewable energy resources "is cluttered with a number of institutional obstacles" (p. 20). Despite this statement, Jerry Yudelson labels as a "mythlike assumption" the belief that legal and institutional barriers prevent the development of renewable resources. Several contributors suggest the desirability of tax subsidies designed to stimulate conservation and greater use of renewable resources. In contrast, Mead stresses that past subsidies have seriously distorted the marketplace and have encouraged excessive energy consumption.

A second theme focuses on the level of government best suited for energy policy making. Daneke and some others favor decentralization and increased reliance on state and local governments. Many other contributors strongly disagree. Douglas Jones believes that decentralization has run its course; Richard Worthington sees advantages in greater centralization. In the words of Henry Harmon, "One of the seeming tragedies of the current energy crisis is that it is occurring at a time when financially strapped municipalities are least able to respond with effective . . . programs" (p. 97). In Lagassa's opinion, state control over the siting of power plants represents a "suboptimal administrative arrangement" (p. 178). Moreover, according to Hanna Cortner, state and local response to the current energy situation has not been noteworthy.

These examples of seeming contradictions are not isolated. One author claims that Congress has abdicated policy making to the Department of Energy; another observes that the department really has very little authority or responsibility. Sharp disagreement exists even about the causes and definition of the energy crisis.

In sum, if policy makers seek clarity and consistency, they are not likely to find it in this book. For others, their reading should be highly selective. They may wish to focus on the appealing chapters by Marc Ross, Sumner Myers, and Janet Johnson and Richard Sylves.

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Proportional Representation in Presidential Nominating Politics. By Paul T. David and James W. Ceasar. (Charlottesville: University Press of Virginia, 1980. Pp. xv + 298. $15.95.)

Prior to the start of the 1976 presidential nomination campaign, politicians, journalists, and political scientists alike were predicting the first brokered convention since 1952. For the Democrats, in particular, the evidence seemed to be overwhelming. First, a larger number and wider assortment of presidential contenders was expected because of the absence of an incumbent, the proliferation of presidential primaries, and the reformed delegate selection process. Second, and more importantly, the adoption of proportional systems of delegate allocation by most primary states and by all caucus states would disperse delegates widely among these many contenders, making a majority difficult to attain. As is often the case, however, the experts were proven wrong, and Jimmy Carter easily managed a first-ballot nomination. What did the experts miss? In *Proportional Representation in Presidential Nominating Politics,* Paul T. David and James W. Ceasar shed some light on this and other questions pertaining to the use of proportional representation in the delegate selection process.

The book is divided into two parts. Part 1 begins by tracing the history of proportional representation from the tumultuous 1968 convention, through the reforms of the McGovern-Fraser and Mikulski Commissions, and ends by describing the more recent recommendations of the Winograd Commission.

David and Ceasar then proceed to analyze the effects of proportional versus plurality or loophole (winner-take-all by district) systems of delegate allocation by recomputing and comparing 1976 delegate outcomes under uniform proportional and plurality plans. Two of the major loophole states, New York and Pennsylvania, are omitted from the analysis because official election returns did not indicate candidate preferences of defeated delegates.

For the Democrats, the findings confirm the expected. Proportional systems tended to fragment delegates among many contenders, giving a larger percentage of the total delegates to weaker candidates and a smaller percentage to front-runners. Plurality systems, on the other hand, tended to concentrate delegates among the leaders and to short-change weaker or fringe candidates.

Given only a two-man race, and the exclusion of New York and Pennsylvania from the analysis, the differences on the Republican side are much smaller and run counter to expectations. Surprisingly, Ford was weakest and Reagan was strongest under the actual mix of proportional and plurality primaries. Under either uniform proportional or plurality systems, Ford would have gained about 20 delegates and Reagan would have lost about 20.

The authors note that the findings for the Republicans would revert to form if New York and Pennsylvania could have been included in the analysis. With New York's and Pennsylvania's delegations included as pro-Ford instead of uncommitted, the majority candidate (Ford) would have received a major boost from a pure plurality system. Under pure proportionality, on the other hand, Reagan (the minority candidate) would have clearly benefited, since he probably would have contested both states and probably would have won a share of both states' delegations. The authors further postulate that "if Reagan had received a share as small as 24 percent in New York and Pennsylvania—and if all other things had remained the same—he would have been the nominee" (p. 56). As David and Ceasar readily admit, these are mighty big "ifs," but they do not detract from their more general conclusion that proportionality "assists" and plurality systems "penalize" the conservative minority within the Republican party.

The authors close part 1 with a discussion of the possible effects of proportional representation on delegate selection and convention decision making. They argue that while proportionality fragments support and therefore increases the likelihood of brokered conventions, the recent rules changes have also generated a countervailing tendency that serves to promote first-ballot nominations. The new rules have tended simultaneously to weaken parties and hence to decrease the number of uncommitteds, and to increase the focus on candidates and hence increase the number of available candidate delegates. As a result, candidates can now win delegates in states where previously they could not do so, and this, the authors contend, reduces the chance of multi-balloted conventions.

In addition to promoting a candidate focus, the authors also argue that proportional representation could also produce a constituency focus, and that, as a result, candidate blocs and constituency blocs could form the new bargaining units at national conventions.

Part 2 consists of case studies which examine in detail the 1976 Democratic and Republican delegate selection process in three primary states (Massachusetts, Texas, and Illinois), two caucus states (Oklahoma and Virginia), and one primary-caucus state (Connecticut). Since they were written by six different authors, they vary somewhat in quality, but each in its own way provides useful insights into different aspects of the delegate selection process or into the problems state parties face in trying to comply with national party guidelines that are sometimes inconsistent and that always conflict with state party goals.

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"Bigotry! Ethnic, Machine, and Sexual Politics in a Senatorial Election. By Maria J. Falco. (Westport, Conn.: Greenwood Press, 1980. Pp. xvi + 200. $18.95.)

This book is the descriptive analysis of a senatorial primary campaign in Pennsylvania and its aftermath. Three disparate phenomena or variables are analyzed: (1) the awakening of ethnic political consciousness of Italian-Americans for greater recognition in the political arena; (2) the allegedly latent fact of political bias against women; and (3) the continuing decline of city-based and state-organized political machines.

In 1964 Republican Hugh Scott ran for reelection to the Senate. The Democrats had endorsed Michael J. Musmanno, a state judge, as their senatorial nominee. Having failed to obtain their preprimary endorsement, Genevieve Blatt, Pennsylvania Secretary of Internal Affairs, contested the seat in the closed Democratic primary. There is no doubt that all three of the factors presented previously were present in varying degrees, although Maria Falco fails to support her assumptions with empirical evidence. Therefore, the question remains as to whether Blatt's defeat was a result of her sex or whether she was the target of revenge by disgruntled Italian-Americans who appear to have resented being underrepresented in statewide political offices. Whatever sexual or ethnic motivations were present, one could conclude that the party organization did not actively support its candidate because the primary wounds never healed. The strongest evidence for Blatt's defeat seems to lie here.

It may be difficult to argue that Blatt was discriminated against because she was a woman. After all, she did win the 1964 Democratic primary. Prior to that election she held the elected office of Pennsylvania Secretary of Internal Affairs from 1955-1967, defeating a number of men competing for the position. Blatt rose through the Pittsburgh Democratic organization into state politics, but one might readily concede that women were only acceptable for lower elected jobs in the organization. However, in 1964, another woman, Democrat Grace Sloan, ran for Auditor General. In the two areas where discrimination is charged, Philadelphia and Allegheny County (Pittsburgh), Sloan had a plurality of 317,059 and 158,537, respectively, while Blatt's plurality was 142,324 and 17,115, respectively. Nor does the fact that Blatt lost by 70,000 votes while President Johnson won by 1.4 million mean much in Pennsylvania. In the five presidential elections since 1952 in which a Senate seat was simultaneously selected, Pennsylvania voters split their tickets four times; and in the four gubernatorial elections since 1958 in which a Senate seat was simultaneously contested, the voters divided each time. Consequently, electing a candidate from one party to the Senate and the other to the presidency or governorship is not an unusual phenomenon in Pennsylvania.

The evidence pointing to Italian reaction to Blatt's candidacy is stronger. Senator Clark, Blatt's ally, generated the bigotry issue in his open fight with Musmanno while Hugh Scott fostered it in the general campaign. Blatt was to lose reelection to Internal Affairs in 1966 and Clark lost his Senate seat in 1968. But the available statistics in 1964 show that in every instance the black and Jewish wards repudiated Blatt to a greater degree than did the Italian ones. A number of post-election interviews revealed that the bigotry issue had an effect but did not in itself lose the election for her.

The most important contribution of the case study is the activity of the political machine in a factional primary battle. Blatt bucked the machine endorsement and Clark had a reputation of mere toleration for the organization. Blatt ran a weak campaign with no overall campaign manager. It would not be the first or even the last time that a party organization geared up for one candidate only to be disappointed with another. The lengthy process of court deliberations, the inadequacies of local election officials to deal with the problem, the design of the ballot itself which was expertly shown to be used in the past to benefit certain local candidates militated against Blatt's election. More importantly, as Falco suggests, the demise of the Philadelphia and Pittsburgh Democratic machines has resulted in growing numbers of independents, reform and anti-organization candidates. The American political party is more enfeebled today than in 1964. What this case study demonstrates is that state and local parties face a transition period as social forces intermix to form new types of political organizations.

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The Irony of Vietnam: The System Worked. By Leslie H. Gelb, with Richard K. Betts. (Washington, D.C.: The Brookings Institution, 1979. Pp. viii + 387. $14.95, cloth; $5.95, paper.)

The Vietnamese War was America's longest war; it was also America's greatest foreign policy failure. For more than 15 years, political analysts have sought to explain why and how the U.S. became involved in that conflict and to draw lessons from the experience. And given the awful and awesome consequences of the war and of the communist revolutions that followed it in Indochina, they will continue to do so for years to come.

In *The Irony of Vietnam: The System Worked,* Leslie Gelb and Richard Betts have written the most comprehensive and most definitive account yet of the forces that put America in Vietnam and kept it there until 1975. Successive chapters of the book present (a) the range of existing and competing explanations for the U.S. involvement; (b) detailed accounts of U.S. decision making on Vietnam from the Roosevelt administration to the Johnson administration; (c) analyses of U.S. policy makers' goals, stakes, and calculations of the means for winning the war; (d) analyses of U.S. policy makers' perceptions, hopes, and "strategy of perseverance"; and (e) a conclusion on the lessons of Vietnam. In making their case, the authors include references to virtually every major scholarly interpretation of the U.S. involvement in Vietnam published in the last generation.

The thesis of the book is that the explanation for U.S. involvement in the war lies in the doctrine of containment and in the American political consensus which supported that doctrine. American presidents believed in the containment doctrine; the commitment to maintain a non-communist Vietnam was merely the natural and logical consequence. And in adhering to this containment logic, the presidents also adhered to the political consensus in the nation. With regard to the means by which to maintain a non-communist Vietnam, the political consensus from 1950 to 1965 was to use the "minimum necessary," i.e., growing quantities of military assistance and military advisors; after 1965 keeping a political consensus meant using the "maximum feasible," i.e., massive but still limited military operations. In brief, both the purpose of the war and the way it was waged, both the goals and the means, were the product of the political consensus on the containment doctrine.

For most political scientists, the special quality, indeed the great glory of the American political system as designed by the Founding Fathers two centuries ago is its central tendency toward consensus policies, toward bargaining and compromise in which no one wins everything and no one loses everything. For Gelb and Betts, the U.S. policy in Vietnam was from first to last a product of consensus; it is in this sense that "the system worked."

This thesis stands in opposition to several other explanations for the U.S. involvement which the authors discuss. They especially seek to refute explanations which focus on "bureaucratic politics" and "the slippery slope," which see successive presidents being led by bureaucratic misinformation into an "inadvertent descent into unforeseen quicksand" (pp. 16-25). Here, their analysis is similar to, but more complicated than one put forward by Daniel Ellsberg in an essay, "The Quagmire Myth and the Stalemate Machine" in his *Papers on the War.*

In summary, this is a substantial and serious book—subtle and perceptive in its analysis, wise and comprehensive in its understanding, and prudent and sensible in its conclusions. No political scientist interpreting the Vietnamese War can ignore it.

There are some anomalies in the book's thesis, however. There were times when the containment doctrine was applied with considerable ambiguity. For example, U.S. policy makers after 1948 saw a unified although communist Yugoslavia under Tito as a means to contain the greatest communist power, the Soviet Union, in Southeast Europe. By itself, the containment doctrine does not explain why these same policy makers did not see a similar potential for a unified although communist Vietnam under Ho Chi Minh as a means to contain the great communist powers, the Soviet Union and China, in Southeast Asia (or indeed why it took them so long to see the potential for China itself to contain the Soviet Union). Conversely, the containment doctrine by itself could have led President Eisenhower to undertake military intervention in northern Vietnam (Tonkin) in 1954 to prevent its fall to the communists.

In the chapters giving detailed accounts of U.S. decision making on Vietnam, the authors give evidence that political considerations led policy makers to interpret containment to mean different things at different times. This suggests that the central variable was really the political consensus in any particular period. But here there are further anomalies. Although the authors' basic model has the president almost totally circumscribed, indeed contained, by the political consensus, the book presents a good deal of evidence for each administration from Truman to Johnson that the president and his advisors actively shaped that same political consensus in regard to Vietnam, either by acts of commission (e.g., public addresses) or omission (e.g., not putting crucial information before the public). This was most obviously and decisively the case in 1964, when Lyndon Johnson and his advisors repeatedly deceived the Congress, the press, and the public. The authors seem to recognize the importance of these actions for their thesis, and they argue that Johnson himself had not yet fully decided upon a war. But it is clear that a different presentation of the issue at that time by the administration would have produced a different political consensus. In 1964, there was a political consensus to attempt to maintain a non-communist Vietnam with means such as military assistance and advisors; there was certainly no consensus in the public to go to war in Vietnam.

Where, then, was the consensus to go to war in Vietnam? Among the foreign policy elite of the day, sometimes known as "the Eastern Establishment," and particularly among those figures, then wonderfully and ludicrously known as "Wise Men," that David Halberstam wrote about in his book *The Best and the Brightest*. It was *their* system that really worked.

In their conclusion on the lessons of Vietnam, the authors observe that the nature of the American political system, like that of any political system, is to make errors in foreign policy from time to time. The appropriate task is not to design a system that will never make errors, but to improve the American one so that it can quickly recognize and correct them. Here their prescriptions follow logically from their analysis. Americans in the future should avoid congealing analyses into doctrines, such as that of containment; the avoidance of doctrines would permit more intelligent and prudent examination of the realities in particular circumstances. Americans should also avoid congealing politics into consensus; there should be more opportunity for dissent in the Executive and in Congress. In particular, Congress could be more quick and more active in turning around a bad policy. These prescriptions—pragmatism instead of doctrine, dissent instead of consensus, checks and balances instead of executive centralization—amount to making the most distinctive features of the American political system even more distinctive and pronounced, or rather to restoring the system to what it was before World War II. It is an irony of Vietnam indeed that, as a result of the war, the system now works even more the way it was originally designed to do.

JAMES R. KURTH

Swarthmore College

Truman's Crises: A Political Biography of Harry S. Truman. By Harold F. Gosnell. (Westport, Conn.: Greenwood Press, 1980. Pp. xv + 656. $35.00.)

The subtitle of this long book is more descriptive of most of its content than its lead title. It is a copiously researched and largely factual account of the rise and the presidency of Harry S. Truman. It is also and perhaps necessarily a pretty thorough history of events and developments which affected or were confronted by Truman before and during his presidency. Harold Gosnell was for many years a professor of political science at the University of Chicago. He has also held a number of administrative and research positions in the federal government, and has become a prolific writer on public affairs. His primary interests have been politics, political leadership, and voting behavior. These concerns are reflected in the emphases of this book on Truman's politics and his political decisions rather than on his administrative experience and his personal life.

In his preface, Gosnell undertakes to answer his own rhetorical question: why another book on Truman? He points to two diametrically opposed views among the writers about Truman. One regards him as one of the truly great presidents—"decisive, intelligent, modest, discreet, farsighted. . ."; the other considers him "indecisive, dumb, inept, cocky, indiscreet, vulgar, second rate. . . ." Gosnell's declared aim "is to search for the truth, which, [he hopes] to show, lies somewhere between the extremes."

The book proceeds with a mainly chronological description of Truman's growth and maturation. Its first of three parts recounts his life from his birth in 1884 to his succession to the presidency in 1945. It tells of his family background, his early education, his experience in farming, his business experience and ultimate failure, his captaincy in World War I, his beginnings in politics in Missouri which started in 1922, his local appointments with the support of the Pendergast organization which culminated in his nomination and election to the U.S. Senate in 1934. This part of the book includes an account of his chairmanship of the Senate's Special Committee to Investigate the Defense Program during World War II, better known as the Truman Committee. This was the principal source of his national recognition that led to the presidency.

The book's second part, appropriately entitled the "Accidental President," deals with the almost unbelievable problems confronting a new president who had not been familiarized with the basic information and policies of the administration in the midst of a major war. Truman's growing confidence and optimism, his knowledge and appreciation of history, and his combativeness during the years of 1945 to 1948, remain among his most impressive characteristics.

The third part of this book concerns the "President in His Own Right," the term which expresses his surprising reelection in 1948. It deals with his actions with respect to NATO, China, McCarthyism, a variety of domestic social programs, his prospective successor—Eisenhowr or Stevenson—and ultimately to the rather unpleasant transition to Eisenhower.

The closing chapter (number 43) reverts to the question suggested by its title and raised in the preface with particular reference to Truman's decisions. In this chapter Gosnell also seeks, through a variety of quite inadequate measures, to evaluate one president against another. Comparisons of this kind are always speculative; few presidents can be compared with others because the circumstances and the challenges they face so differ. In his closing chapter, Gosnell describes Truman's decisions in three basic categories: pre-presidential, foreign relations, and domestic decisions. Although his assessment is not altogether clear, his account indicates that, in his opinion, Truman was decisive and wise: a "near-great president."

This book is the product of a tremendous amount of research. It is in the main accurate and complete. Those who, like this reviewer, remember the Truman years, will read it with nostalgia and will learn some things they did not know or have forgotten. It is a first-class reference work. But it is long and impersonal reading. And one concludes it without feeling that one knows much more about Harry S. Truman as a man than one knew before.

FREDERICK C. MOSHER

University of Virginia

Machine Politics in Transition: Party and Community in Chicago. By Thomas M.. Guterbock. (Chicago: University of Chicago Press, 1980. Pp. xxii + 324. $20.00.)

Sociologist Thomas Guterbock addresses some large and significant questions in his study of contemporary machine politics in Chicago. Rather than providing simple description, Guterbock attempts to determine the factors which account for effective organizational persistence. In particular, he explores the linkage between the service activities of a ward club and voting support for the party's candidates.

Squarely in the tradition of community-based research, Guterbock combines survey methods with participant-observation in an intensive case study of one ward on Chicago's North Side. The ward, not specifically identified, is characteristic of most wards in Chicago—the Democratic party is the only game in town. The ward is quite heterogeneous, possessing sizeable numbers of white upper- and middle-class residents, a diverse collection of low-income ethnic and social groups, and a stable white working-class population.

Guterbock lived in the ward from 1972 to 1975; for half of that period, he was active in the regular Democratic organization as an unpaid assistant to the Democratic alderman. As such, he had access to the alderman's files from which he systematically drew a client sample of 111 names. A systematic sample of 170 names were drawn from the ward's registered voter list. For most of the analyses these two samples were combined, producing a total of 160 completed interviews. Appendices usefully contain the full interview schedule, item marginals, plus descriptions of the sampling, interviewing, and multivariate analysis procedures employed.

Originally, Guterbock hypothesized that the organization derived continuing voter support through certain kinds of social exchange mechanisms. Material incentives would elicit party loyalty from low-income residents. Other urban immigrants, not yet completely assimilated into the community, would provide support in exchange for personal and affective ties to the party workers living in the community. However, Guterbock found that the data did not support these hypotheses. Material exchange was useful only in understanding the organization's internal workings: party work insured a public job. The ward club was seen as a complex compliance structure which used various techniques to secure its workers' compliance, many of them being "venal" and basically "undemocratic" in nature. Organizational maintenance required, in Guterbock's terms, this "system of defended venality," with the public being carefully shielded from the essential nature of the organization.

To explain the continued support of the citizenry, Guterbock develops a "community attachment" model which assumes a distinctly different basis for external support. Internally, the Chicago patronage party is seen as "a tightly organized exchange system involving politicians of diverse origins who have self-interested motivations in common" (p. 226). In terms of the external environment,

> the party uses service activities to show that it is responsive to persons who feel the responsibilities of local citizenship. By their substantive and symbolic actions in local public life, the patronage politicians persuade the voters that they are concerned about the local community and that they are acting to advance its interests (p. 288).

Unfortunately, the survey instrument was not adequate to test fully this model which emerged during the course of the study.

The study has other limitations. The sample size is quite small. Combining two discrete samples for data analytic purposes is questionable. Little effective use is made of the qualitative data derived from participant-observation. Finally, the representativeness of this particular ward and organization must be questioned, and thus the generalizability of the study's findings.

Nevertheless, as an exploratory study this is a worthy effort. Guterbock tested some widely supported notions and found them wanting. As a consequence, he developed an alternative model for which he finds some confirmation. He provides a plausible explanation for the continuing viability of a political organization in a changing environment, a model worthy of receiving systematic and more refined testing in other studies. This is Guterbock's chief contribution in *Machine Politics in Transition* along with providing other young researchers with an example of careful, systematic, and self-conscious social inquiry.

RICHARD M. JOHNSON

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The Rewards of Public Service: Compensating Top Federal Officials. Edited by Robert W. Hartman and Arnold R. Weber. (Washington, D.C.: The Brookings Institution, 1980. Pp. xi + 238. $11.95, cloth; $4.95, paper.)

The head of the Social Security Administration, who will oversee 77,000 employees and annual outlays of $136 billion this year, receives an annual salary of $52,750. The Aetna Life and Casualty Company has half that number of employees and assets of $30.2 billion, yet it is the largest diversified financial company in America. It paid its chief executive—in salary and bonuses alone—$541,000 last year.

For a variety of reasons, specialists in public affairs have begun to worry that the total compensation system for top federal executives is insufficient to attract and hold expert public managers in their jobs. The obstacle to reform resides in the fact that executive salaries at present are pegged to the salaries that congressmen and senators vote for themselves—in fact, for the new Senior Executive Service, they are pegged some $10,000 below the remuneration of lawmakers.

Against this background, the Brookings Institution in 1978 assembled a conference of academics and practitioners to examine one of the few issues in public administration that is automatically volatile—the pay of top federal officials. The resulting book consists of seven papers by eight of the participants.

Without much difficulty, the participants demonstrate that the pay issue is "politically awkward" (p. 87). As Louis Fisher reveals in his history of pay adjustments in Congress, "A vote on a pay raise is uncomfortable and was meant to be uncomfortable" (p. 50). This lesson is reinforced by the failure of the decade-long effort by Congress, beginning in 1967, to depoliticize the process by assigning the compensation issue to an independent nine-member quadrennial commission whose "impartial" recommendations, funneled through the president, would become law if Congress did nothing.

Informed of the past, the conferees set out in search of solutions for the future. The solutions they propose are professional and managerial—in two senses. First, the conference was dominated by college-educated professionals and managers—the only subgroup of the U.S. population, as Roger Davidson points out in a paper that begins with a summary of public attitudes on the subject, that are receptive "to more generous compensation for government managers" (p. 71). Not surprisingly, the major recommendation of the book is that executive and judicial salaries be disconnected from congressional pay—for the purpose, one assumes, of letting executive and judicial pay rise more rapidly.

Once removed from congressional hesitation, the determination of executive and judicial salaries would become, in the eyes of the conferees, a management problem. "In its pure form, the issue of the proper compensation . . . is a subset of the general problem of wage and salary determination" (p. 5), subject to the precepts of public administration and "the economic theory of occupational choice" (p. 101).

Public administrators prescribe different salary levels for different levels of responsibility. Congress, however, has acted otherwise. By being more generous toward low- and mid-level bureaucrats than toward itself and, by linkage, top executives, it has created a salary squeeze, most acute for members of the Senior Executive Service. Their total salary differentiation is being compressed into a few thousand dollars by the swelling magma of five million civilian and military personnel pushing up against the dome of congressional pay.

The attempt to think about total executive compensation (present, future, material, and psychic) in terms of the economics of supply and demand, comparability, net advantage, lifetime income and market differentiation might have some strange consequences. There is no economic rationale, for example, for paying the Secretary of Defense, who manages an institution three times the size of America's largest corporation (the telephone company) the same salary as the Secretary of Housing and Urban Development, whose organization employs the same number of persons as Anheuser Busch.

It is still possible, of course, to fill top executive posts by appealing to the candidate's sense of public service (and the promise of future financial payoffs in the private sector). Edward Lazear and Sherwin Rosen, in their sensitive paper on the economic aspects of compensation, show how this tends "to produce a government of the young and inexperienced . . . with high turnover and short tenure in office" (p. 112).

Some readers may find the book unnecessarily naive. It is hard to imagine our lawmakers embracing a proposal to depoliticize executive and judicial pay and let those salaries slip past their own. Conversely, it may be just as naive to believe that Congress can continue to present government executives with compensation packages that are perceived as substandard and contrary to the economics of the situation, and still expect high levels of performance.

HOWARD E. MCCURDY

American University

The Truman White House: The Administration of the Presidency, 1945-1953. Edited by Francis H. Heller. (Lawrence: The Regents Press of Kansas, 1980. Pp. xxiii + 247. $15.00.)

Three years ago the Harry S. Truman Library Institute for National and International Affairs assembled in Kansas City 22 persons who had been active participants in Truman's presidency. Their two days of discussions and the several short papers or statements written for the conference or contributed from earlier accounts by participants have been edited into this book by Francis H. Heller, who also had helped coordinate the conference.

Such a conference and its record are a valuable supplement to the static research materials housed at the presidential libraries. The papers of each administration in the respective collections are the basic contemporary materials, and the oral histories are excellent commentaries and explanations soon after the fact by participants and observers. This conference by administration participants with the added perspective of time and other presidencies has provided a different kind of material. The resulting book is an approximation of the "Heavenly Discourse" which has allowed the former staffers to discuss and argue about salient features of the Truman administration in the light of the succeeding Eisenhower, Kennedy, Johnson, Nixon, and Ford administrations. In fact, all of the participants had been given copies of Stephen Hess' book, *Organizing the Presidency* (Brookings, 1976) and were asked to react to 15 questions (not provided in the Heller book, unfortunately).

Editor Heller warns the readers that "Historical perspective is often dangerously clouded by hindsight; it is easy to say today that this or that should have been done differently" (p. xxiii). Nearly a quarter-century of intervening years has also clouded details in some of the participants' accounts as well. Quite naturally, all of the commentators, whether political appointees or careerists, tended to be very laudatory of the Truman presidency and President Truman as well. It is not an unbiased account.

Each of the four discussion sessions in the two-day conference was scheduled to cover a specific topic: the cabinet, the early period of the White House staff, the latter period of the White House staff, and the Executive Office Agencies.

In the papers and reported discussions, there was a broad consensus that the Truman administration was not top-heavy with White House staff, nor was the staff on top of the agencies, as appears to be the case in some succeeding administrations. There are repeated statements that White House staff did not come between cabinet officers and the president.

There was no agreement on whether Truman did have a real chief of staff, whether John Steelman, who carried the unique title, "Assistant to the President," or Clark Clifford or Charles F. Murphy were ever de facto chiefs of staff in the contemporary notion. Certainly, all agreed that even John Steelman was not a chief of staff in the Sherman Adams or Bob Haldeman sense. The morning staff sessions presided over by the president, who handed out assignments and sought staff views, gave access to the president even to the junior staffers and gave them direct knowledge of what the president wanted. Morale was high, and the president and staff had a high level of trust and confidence. The conference participants, 24 or more years after their involvement, discount Stephen Hess' assertion that the staff inter-position between cabinet and president began in the Truman era. Charles F. Murphy said about the idea of White House staff trying to control operations in the departments, "I think it is fair to say that during the Truman administration there was none of that. In the first place, the president regarded it as highly improper; and in the second place, I do not think the staff had any such ambitions or desires" (p. 64). There were more likely to be oversight duties by White House staff with regard to some of the smaller, non-cabinet department committees and agencies.

All of the participants seemed to agree that President Truman had a keen sense of history, a sharp comprehension of constitutional prerogatives of the respective branches, legislative and executive, which should not be violated, as well as a right of the departments not to be supervised heavily. Truman gave direct access to cabinet members and all key staff. Matthew Connelly, his appointments secretary, controlled the timing but not the ultimate access to the president via phone calls or personal meetings.

It is unfortunate that ill health or conflicting obligations kept away the other 18 invited participants such as Clark Clifford and James E. Webb, who would have contributed to the content and style of the discussion. Undoubtedly, the participants in the conference and the session moderators concentrated upon aspects of the Truman White House, one would not have been so likely to have paid attention to before Watergate. There is a great deal of duplication in this book of earlier oral histories and other accounts, but the advantage of a post-Watergate view outweighs the inevitable historical fallacies that occur so long after the fact. What the Truman Institute and Francis Heller have accomplished in this publication should encourage the other libraries and their supporting foundations to attempt similar ventures. Calling together veterans of past administrations to reflect systematically upon their past roles and activities can be useful. Directed discussion, interpretation, introspection, and disagreement among participants can add useful insights for scholars of the presidency. Enlightening footnotes to history have been created by the conference and have been edited into a usable format, more accessible than stray interviews by individual scholars which may never make it into print or into the Truman archives.

W. LANDIS JONES

President's Commission on
White House Fellowships

Why Policies Succeed or Fail: Sage Yearbooks in Politics and Public Policy, Vol. 8. Edited by Helen M. Ingram and Dean E. Mann. (Beverly Hills, Calif.: Sage, 1980. Pp. 312.. $20.00, cloth; $9.95, paper.)

The Policy Studies Organization's 1980 Yearbook raises a crucial question—why politics succeed or fail. The question is the essence of public policy analysis as that area of teaching and research has developed in the political science discipline in the past decade.

Except for the series editor's introduction and opening and concluding chapters by the editors of the volume, the eighth yearbook in the organization's series leaves the impression that political scientists, by and large, are not yet ready to answer with systematic research the conceptual issues raised in the volume. Most of the articles seem not to have been written for the yearbook's theme and the selection of articles fails to round out the conceptual concerns of the editors.

As a result, the reader gets an unsatisfactory answer to the question of policy success and failure. In the introduction, Helen M. Ingram and Dean E. Mann speculate on some of the critical determinants in policy failure: intractability of the environment, nature and extent of available resources, and operant political constraints. There is a serious oversight, however, of two major areas that previous research has demonstrated as critical factors in successful and unsuccessful policy making. Little attention is given by the editors or the contributors to citizen participation and clientelism which emerged as a critical determinant of welfare and poverty policy failure. Nor does the yearbook address alternative policy delivery, which economists and public choice theorists raise as a potential for the design of successful solutions from previous policy failure.

In one of the more important contributions, Ingram and R. Kenneth Godwin pick up on the recent theme of single-issue politics to develop interesting insights into the role of political institutions in policy failure. The other articles are too esoteric and unfocused on the yearbook's theme to contribute much to a theory of policy success or failure or to alert the inquiring reader and aspiring researcher to critical variables which affect policy outcomes.

In addition to policies, one might wonder why yearbooks fail or succeed. The Policy Studies Organization, with the assistance of Sage Publications, has provided a useful forum for the publication of policy-oriented research in the discipline. Through the yearbook series, policy researchers have found an outlet for sharing their research interests and findings with others, with shorter than typical time lags. A lot of ground has been covered: what is policy; how policy is formulated, implemented and evaluated; urban dimensions of policy; and how policy is made in a federal system. Editors of future yearbooks might consider commissioning research which addresses the topic. Otherwise the Policy Studies Organization may need to put off asking such crucial questions as why policies are or are not successful, until the answers are known.

J. OLIVER WILLLIAMS

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Public Entrepreneurship: Toward a Theory of Bureaucratic Political Power: The Organizational Lives of Hyman Rickover, J. Edgar Hoover, and Robert Moses. By Eugene Lewis. (Bloomington: Indiana University Press, 1980. Pp. x + 274. $22.50.)

This is a fascinating, original, and seminal book that deals with the impact of personal character and style in public administration. Eugene Lewis reviews the careers of Hyman Rickover, J. Edgar Hoover, and Robert Moses to illustrate his thesis that public organizations may be shaped more by the personalities and orientations of individuals within them, particularly those at the top, than by formal organizational characteristics or even informal organization.

Lewis uses the examples of Rickover, Hoover, and Moses to develop a theory of public entrepreneurship. The public entrepreneur is a "person who creates or profoundly elaborates a public organization so as to alter greatly the existing pattern of allocation of scarce public resources. Such persons rise and succeed in organizational and political milieus which contain contradictory mixes of values received from the past. Public entrepreneurs characteristically exploit such contradictions" (p. 9).

Like elected politicians, public entrepreneurs seek power and status within and without government. Because of the vast resources of the bureaucracy, public entrepreneurs often have a broader influence upon public policy than their elected counterparts. Public entrepreneurs dictate their values to the other branches of the government, and are only intermittently controlled by Congress and the president. The potential power of the leaders of public organizations stems from their tenure and their ability to draw upon the resources of the organization to develop expertise that may become virtually unchallengable. Congress did not dictate, for example, to Admiral Rickover when it came to matters of nuclear technology concerning the atomic submarine. When J.. Edgar Hoover testified before congressional committees, he was treated with deference that was accorded to virtually no other government official. Rickover and Hoover had tight control over their respective organizations, and had become the most skillful manipulators of individuals and rules in the federal bureaucracy. They had become virtually a law unto themselves concerning matters under the jurisdiction of their agencies.

Lewis stresses the fact that in the final analysis public entrepreneurs are supreme politicians who do not have to stand for election. The three men who are the focus of this book "had the vision and the opportunities to make that leap from the confines of formal rule-bound bureaucracy into the political arena, without ever having to divest themselves of their grasp on organizational mandates and other resources. They were, in effect, a new kind of politician, one successfully adapted to the pressures of 20th-century society" (p. 239).

Lewis concludes that the power of public entrepreneurs presents a gloomy picture to those who believe in the ideals of representative democracy. These top level bureaucratic-politicians "reduce the significance of the traditional political system and thereby alter the face of democratic government and politics" (p. 241).

This book lays to rest once and for all the common notion that the administrative branch of the government is stagnant, and composed of faceless bureaucrats who are generally without ambition and incapable of innovation. The book is must reading for all scholars and students of political science and public administration.

PETER WOLL

Brandeis University

Mr. Justice Black: Absolutist on the Court. By James J. Magee. (Charlottesville: University of Virginia Press, 1980. Pp. xv + 214. $22.50.)

Shortly after he announced for the first time that freedom of speech is absolute, Hugo Black received a letter from a critic who said, in effect, that he had been wanting to express his opinion of him for a long time but had been afraid of the libel laws. "Now, I am at last free under your interpretation of the First Amendment to express my precise opinion of you. Mr. Justice, you are a sonofabitch" (Hugo Black, Jr., *My Father,* Random House, 1975, p. 184). The source of this anecdote is Black himself and he nowhere suggests that his critic incorrectly interpreted his rights under Black's absolutist thesis.

James Magee's book on Black is a detailed analysis of this thesis—how it developed, the reasons for its development, the criticisms directed against it, and its practical and theoretical ramifications. The author treats Black's constitutional jurisprudence from 1937 to 1971 with particular reference to the First Amendment. That jurisprudence is said to be based on three factors: absolutism, literalism, and Black's reading of history. Literalism refers to Black's belief that the words of the Bill of Rights are to be taken literally—that they mean what they say. Absolutism describes the thesis that all constitutional limitations on governmental power are to be strictly enforced. History is used by Black to reinforce absolutism/literalism and to justify Black's constitutional interpretations.

While conceding that Black's absolutist interpretations of the First Amendment are possibly justifiable, Magee makes and supports a number of claims that tend to dilute, if not destroy, such possibilities. Thus, during the period in which Black subscribed to the absolutist/literalist thesis, he (1) made exceptions to the demands of the thesis where he insisted none should be made and failed to explain why, (2) promoted the myth that the breadth of freedom of expression in the First Amendment is self evident and self defined in the words of the Amendment, and (3) ascribed an historical meaning to freedom of expression "strikingly short" of that justified by a fair reading of historical materials (p. 50).

On the subject of change—the author argues convincingly that Black was a balancer of interests in the forties and became an absolutist subsequent to 1951. He gives considerable attention to the alleged change in Black's behavior to which many observers have alluded in the period 1964-1971. Magee is decidedly unhappy with some of the explanations offered by these commentators. He is particularly disdainful of Glendon Schubert's 11-page treatment of Black in his *Constitutional Polity* (Boston, 1970). Agreeing with Jeffrey Morris that Schubert accuses Black of becoming senile (*Journal of Politics* 34: 296), Magee finds Schubert "willing to explore any hypothesis under the sun before he attaches any significance to Justice Black's determination to adhere to the strictures and doctrines that he synthesized into a judicial philosophy" (p. 149). Noting at one point that Schubert's fragmentary and "nearly non-existent evidence . . . refutes rather than supports his conclusion" (p. 149), the author suggests that "Schubert's unsupported conjectures flow from an undisguised dislike of the results that Justice Black derived in the late 1960s, from his literalist construction of the Constitution" (p. 150). The alternative explanation clearly preferred by Magee is that Black's libertarian attitudes waned in the period 1964-71 and that this, coupled with his allegiance to the demands of his judicial philosophy, accounted for any behavioral change that might have occurred.

While I am inclined to agree with the author's conclusions regarding the 1964-71 period (my own research is supportive here) and most of his other general lines of argument, Magee's treatment of Black's critics struck me as a little incautious. I suspect that some of these critics would more than quibble with the characterizations of their criticisms as well as with the context in which their comments were presented for analysis. And the author's comments on other observers of Black are not always supported by probative evidence. For exampled, while Schubert may or may not have "conjectured" that Black became senile in the period 1964-71, the author's evidence for the proposition that such conjecture was a product of Schubert's dislike for the substance of Black's decisions is unconvincing. In any event, the hard treatment of Black's critics pales somewhat when it is noted that Magee and Black's protagonists eventually reach the same general conclusion —i.e., that the absolutist thesis cannot stand close analysis.

In general, Magee has provided us with an excellent overview of Black's jurisprudence. The book is well crafted. The scholarship is solid. Most of the arguments presented are well supported and, on the whole, convincing. One can say this without assessing the importance of the absolutist thesis for American jurisprudence. In one sense, if the absolutist thesis is taken literally, it is indefensible. If it is not taken literally, the theoretical significance attributed to it by the literature is unwarranted. Though Magee clearly shows the thesis to be one which attracted the attention of large numbers of scholars, the significance of its impact on constitutional law and jurisprudence holds a potential for further development.

S. SIDNEY ULMER

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Politics As Communication. By Robert G. Meadow. (Norwood, N.J.: Ablex Publishing Corporation, 1980. Pp. xvi + 269. $24.95.)

With this book, Robert Meadow accomplishes three important tasks. First and foremost, he analyzes political processes and institutions from the perspective of communication, showing how changes in communication variables alter the shape of politics. This differs from the traditional approach of treating communication as a dependent variable. Secondly, Meadow reviews selected major works in the literature about political communication and points out their theoretical and analytical shortcomings. Finally, he maps out areas for research and provides a model to guide scholars from a variety of disciplines in their future explorations of the field of political communication.

Politics As Communication is divided into four parts. Part 1, entitled "An Overview of Politics as Communication," sets the stage with succinct definitions of basic terms and brief, critical reviews of past works in the field. Six major theories of political communication are described, along with thel models they have spawned, including a model composed by the author which attempts to synthesize major features of prior models. The discussion is enriched by a series of well-executed diagrams.

Part 2, "Communication Perspectives on Political Concepts," examines how communication in its various forms affects eight major political processes. Here Meadow focuses on conflict, integration, development, participation, power, influence, authority, and political socialization. Each concept is probed from a variety of perspectives. For example, in discussing the role of communication in political conflicts, Meadow outlines how communication may avert or initiate conflict, how it attracts or repels a variety of participants to the fray, and how it is crucial in terminating conflict.

In Meadow's analysis of power, he shows how it involves not only the ability to control communication channels to send and suppress messages, but also requires that aspirants to power place themselves at the strategic hub of communication.

Meadow's chapter on political participation appropriately includes protest movements, as well as more routine forms of participation, on the grounds that protests of all types are a major form of communication for those lacking linguistic skills and financial and organizational resources. While political participation is viewed broadly, political socialization is treated from a limited perspective. Meadow focuses on childhood socialization, largely omitting the more important questions of the role of communication in adult socialization and re-socialization.

In part 3, "Communication in Political Institutions," Meadow examines the impact of internal and external communication on the functions performed by major governmental and private-sector institutions, such as legislatures, political parties, and mass media organizations. He calls attention to a variety of communication problems that plague such institutions. Examples are communication overload, which requires delegating much information screening to people at the bottom of the decision-making hierarchy, and the problem of facilitating the entry of change-oriented information into the system. Normally, organizational communication systems filter out information that endangers the status quo of the organization.

Part 4, on "Communication as a Political Issue," is a grab-bag of tenuously linked topics. The section begins with a general discussion of language and its meanings in various political contexts. The focus then turns to case studies of French-language policy in Canada and Spanish-language policy in the United States. A chapter on popular culture analyzes the pro- or anti-regime political messages that non-elitist movies, television shows, comics, artistic performances, and the like, convey to the general public.

The final two chapters of part 4 deal with constraints and regulations of the mass media information flow and with the uses of communication by government for public information, public relations, and propaganda. Besides formal government regulation of the mass media, Meadow discusses informal constraints springing from the need to make money from mass communication, to comply with demands for privacy and to restrain news reports threatening national security. The book ends with a comprehensive bibliography that should be very helpful to readers who wish to deepen their understanding of the many topics that have been sketched out for them.

As this summary of *Politics As Communication* indicates, the book covers a vast stretch of intellectual territory. It pays the price for such breadth in lack of depth, including a failure to convincingly establish the superiority of the communication approach over other approaches to political analysis. Nearly every one of the chapters could—and eventually should—be expanded into a book-length study because the communications perspective on politics has become increasingly useful and important.

The second half of the twentieth century, more than any previous period, is an era of communications dominance. It has become easier than ever before to gather politically relevant information, store it, retrieve it, and disseminate it to large numbers of people in written, oral, and visual form. More and more people are participating in the communication process, both as senders and receivers of messages. Communication therefore has become an increasingly pervasive and powerful political force. It lends itself more readily to human control than most other variables in the game of politics. The world's political leaders and major publics are fully aware of the power of communication. Understanding how it works and how it can be used to advantage has become paramount. By mapping out the field in original and stimulating ways, *Politics As Communication* makes a major contribution to filling this need.

DORIS A. GRABER

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National Interests and Presidential Leadership: The Setting of Priorities. By Donald E. Nuechterlein. (Boulder, Colo.: Westview Press, 1978. Pp. xvi + 246. $18.00, cloth; $8.00, paper.)

Although published in 1978, Donald Nuechterlein's book is remarkably pertinent in this election year as it considers the roles of the president and the Congress in defining the national interest of the United States. In reviewing a number of foreign policy problems which faced President Jimmy Carter at the end of 1977, it used a method of analyzing the national interest by which it predicted many of the difficulties facing the President now.

This work would be useful if only for such contributions. But its greater use results from Nuechterlein's effort to explore the concept of national interest more systematically and to promote consideration of national interest in terms of value/cast analysis. Although the idea of the national interest has been explored extensively before, this book provides an orderly process by which the policy maker and scholar may define and evaluate the national interest in specific cases.

Nuechterlein defines the national interest as "the perceived needs and desires of one sovereign state in relation to the sovereign states comprising its external environment" (p. 3). Although this definition excludes elements of the external environment other than sovereign states, seems to reflect the organic theory of the state, and seems to minimize the impact of domestic politics on the national interest, Nuechterlein indicates in the analysis which follows that he has not completely overlooked such considerations.

He, first, categorizes four basic interests: *defense interests, economic interests, world order interests* (distribution of power), and *ideological interests* (protection and promotion of the values of the nation). No priority is established among these types of interests, since each case will require its own order. Such interests are not mutually exclusive, and any case will involve an interrelated mix of the four. Second, he classifies the level of involvement or *intensity* of interest. Some interests involve *survival issues* which carry a threat to the state so serious as to warrant, possibly, an unlimited response. Some are *vital issues* which might call for a conventional force response. Some are *major issues* which are usually settled by diplomacy, and others are *peripheral issues* which are those involving private citizens or economic enterprises but which do not threaten the state as a whole. From these eight considerations, Nuechterlein forms a matrix to analyze a state's national interest in a specific case (p. 11) and then applies the matrix to different states involved in the case (p. 13). He then refines the meaning of *interests* and *intensity* and provides a table (p. 20) which summarizes the various values and costs which must be considered in any determination of policy to satisfy a perceived interest.

Nuechterlein applies his method of analysis to four cases in which presidents of the United States opted for military action abroad. He considers the perceptions of national interest of Wilson in regard to World War I, of Roosevelt in World War II, of Truman at the time of the Korean police action, and of Johnson in regard to Vietnam. He applies it to Nixon's decision to attack Cambodia. And, he considers the then unresolved cases of the national interest in regard to the Panama Canal Treaties, to the effort to separate Quebec from Canada, and to the question of racial policy in the Republic of South Africa. In each case he uses his method of analysis to evaluate the interests of all of the parties involved. Almost incidentally, he has included a chapter on the process of policy making in which he provides a good review of the ways in which presidents have used the National Security Council since its inception and concludes that the National Security Council system had developed under Nixon, Ford, and Carter. But this is hardly a book on the process of policy making. Nuechterlein's analysis concludes that, although presidents exercise the ultimate operational decision of what national interest is in any specific situation, they accept grave political risks if they fail to consult Congress in any matter involving *survival* or *vital* interests.

Refining the traditional concept of the national interest, Nuechterlein applies both historiography and systematic analysis to define the concept more precisely as an operational analytical guide to policy. The work will be useful to anyone wishing a more complete knowledge of the complexities included in the idea of national interest.

WALTER S. SCHOENBERGER

University of Maine

Privacy, Law, and Public Policy. By David M. O'Brien. (New York: Praeger Publishers, 1979.. Pp. xiv + 262. $23.95.)

Sir Henry Maine, the British historian, once wrote that "the movement of progressive societies has hitherto been a movement from [law based on] Status to [law based on] Contract." In feudal times individual rights were grounded in one's status in an elaborate legal and social hierarchy. With the advent of capitalism a new theoretical basis for liberty had to be found in concepts of property and contract. In the late twentieth century social changes as profound as those which destroyed feudalism have begun to separate power from property, compelling philosophers and lawyers to seek a new basis for liberty in concepts of personhood, dignity, and autonomy. Nothing better illustrates this incipient revolution in the jurisprudence of liberty than the Warren Court's treatment of the burgeoning rights of privacy which are the subject of this book.

Unfortunately, the Burger Court is resisting this trend. The evidence is unmistakable, and in drawing it together David O'Brien has made a valuable contribution to our understanding of what Justice Brandeis once called "the most comprehensive of rights and the right most valued by civilized men."

The book begins with a useful distinction between privacy as a "*condition* of limited access" to the "life experiences and engagements" of indivduals (pp. 16, 20), the *law* of privacy (and liberty) which imposes duties of non-interference on potential privacy invaders, and the *rights* of privacy which enable the victims of some of the more serious privacy invasions to obtain compensation and, perhaps, to deter future violations. O'Brien rightly criticizes the tendency of some theorists to justify the condition of privacy and laws contributing to that condition in terms of a single value, such as information control. He also rejects the idea that privacy simply denotes seclusion or withdrawal, that rights to privacy are necessarily chosen or voluntary, or that acknowledgment of privacy interests necessarily requires the recognition of legal rights.

In his second chapter, O'Brien demonstrates that expectations of a Fourth Amendment more firmly grounded in concepts of personhood have not been fulfilled. Without abandoning the Warren Court's rhetoric about protecting persons, the current majority has converted " 'expectations of privacy into expectations of ownership, possession, or occupancy' " (p. 71, quoting 83 Yale L.J. at 1461).

The Burger Court's exposition of the Fifth Amendment right not to be a witness against oneself also remains mired in the utilitarian reasoning and proprietary concepts of the Property Age. In chapter 3 the author expertly dissects the three most common justifications for that right and shows how the Burger Court has tended to ground the right mainly on the narrow utilitarian need to maintain a responsible accusatorial system. Unfortunately, by separating privacy from respect for dignity and conscience, O'Brien ends up conceding that privacy can provide no more than an "ancillary justification" for the Fifth Amendment privilege. Mind-freedom, the right to be left alone in one's religious life, and the right to limit access to one's political and religious beliefs certainly were part of the common law right to be free from all forms of compulsory self-accusation, and cannot be read out of the privacy debate by a few court decisions interpreting the privilege in the context of ordinary criminal cases.

Limited conceptions of what constitute threats to privacy and principles relevant to the law of privacy also mar the remaining chapters of the book. Chapter 4, entitled "Preferred Freedoms and the Rights of Conscience," is not so much about rights of conscience as it is about the tort law of privacy as a shield against persons claiming First Amendment rights and associational privacy in the face of government investigations. Surprisingly, little is said about political surveillance, perhaps the greatest threat to the privacy of beliefs and associations in our time.

Chapter 5 advances the ungenerous theory that the contraception and abortion cases secure liberty, not privacy, as if there did not exist a substantial body of judicial opinions and scholarly commentary asserting a right of personal autonomy over certain fundamental life choices, including decisions to beget children, engage in homosexual activity, refuse medical treatment, or even choose the conditions and time of death. Chapter 15 of Laurence Tribe's *American Constitutional Law* is more helpful precisely because it does not treat this debate as foreclosed by a few Supreme Court decisions.

O'Brien's analysis of the new statutory rights of informational privacy (or fair information practices) is limited in scope to a dissection of the Freedom of Information and Privacy Acts. Unsuccessful attempts to constitutionalize a right of informational control are discussed at scattered points elsewhere in the book and no attempt is made to make conceptual sense out of the principles which underlie a variety of bills and regulations to govern personal data in bureaucratic hands—principles which, like the common law of privacy, may someday receive constitutional recognition.

Despite these limitations, *Privacy, Law, and Public Policy* is a good book—useful, provocative, well documented, and well written. It is worth the outrageous price Praeger has charged to print it.

CHRISTOPHER H. PYLE

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Women's Organizations' Use of the Courts. By Karen O'Connor. (Lexington, Mass.: Lexington Books, 1980. Pp. xi + 157. $18.95.)

This is a timely work. The importance of women's rights, changing patterns of litigation, and concern with single-issue interest group behavior combine to make Karen O'Connor's analysis of court lobbying both fascinating and pertinent. Focusing on "associations that have engaged in women's rights adjudication before the United States Supreme Court" (since 1869), O'Connor traces the development and treatment of women's rights before the nation's highest tribunal and analyzes the factors that shape the course of litigation (p. 9).

Arguing that interest groups use a variety of litigation-related strategies to suit myriad purposes, O'Connor initially considers litigation as a type of political action and then goes on to review the development of women's rights organizations before finally analyzing the use of the courts by these particular groups. The research summarized builds on history and traditional case analysis, though the author does report assessments from interviews conducted with attorneys involved in major court cases. Of particular interest in *Women's Organizations' Use of the Courts* is O'Connor's analysis of alternative litigation strategies, her comparison of women's rights litigation with that of the civil rights movement, and her scrutiny of factors related to adjudication success.

Taking decided aim with Nathan Hakman's argument "that the available evidence does not support a theory of judicial lobbying or planned litigation" (pp. 15-16), O'Connor identified three distinct types of strategy in women's rights cases: "litigation for publicity, direct sponsorship of cases for desired policy outcomes, and participation as friend of the court" (p. 141). Though this variety of lobbying tactics has long been recognized in judicial process, O'Connor's typological outline, her insightful description of sponsoring organizations, and her political critique of their court agendas constitute a significant contribution.

Another important feature of *Women's Organizations' Use of the Courts* is the consistent and appropriate reference to Vose's study of the NAACP. O'Connor draws the reader's attention to the litigation strategy made famous by the NAACP and analyzed by Clement Vose. O'Connor, though, takes pains to emphasize that women's rights litigation is quite different from the NAACP effort. The variety of organizations involved in women's rights, the sometimes contrasting political agendas (a good illustration being the comparative assessment of the National Woman Suffrage Association and the National Consumers' League), and the presence or absence of any of the factors associated with litigation success (e.g., financial resources) make the difference predictable if not inevitable.

O'Connor's analysis of factors ranging from status of attorney to issue focus critically related to court success reveals no surprises. The author's attention to the idea of litigation control in general and to the lobbying of the National Consumer's League in particular, however, illustrate the point effectively. The National Consumers' League focused on the improvement of working conditions for women. NCL members were enthusiastic in their support of protective legislation though this issue focus left the organization in a defensive litigation position. O'Connor outlines the ensuing strategy that NCL adopted and emphasizes the critical role that Brandeis played. Positing litigation control as the only condition of his involvement in NCL litigation, Brandeis worked successfully as NCL counsel. The limited success of Frankfurter and Acheson after him (neither of whom stressed litigation control as forcefully as Brandeis) sharply illustrates O'Connor's analysis of factors related to litigation success.

The lasting merit of O'Connor's book may lie in the questions that stem from the analysis. How have the changing patterns of litigation and recent alterations in judicial response to demands on courts (e.g., restrictions on class action suits) interacted with interest-group strategy in adjudication? Does the rise of issue-focused interest groups combine with the correspondingly exponential increase in issue complexity to make limited and rather dispersed success the only possible goal in lobbying? How do judges and courts respond to "controlled" litigation and how do these factors interact with judicial attitudes and perspectives in decision making?

These questions illustrate the heuristic contributions of O'Connor's work. Combined with the detailed case study of women's organizations and the careful analysis of women's rights cases before the U.S. Supreme Court, this makes *Women's Organizations' Use of the Courts* an important contribution to the literatures of judicial process, interest groups, and women's rights.

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The Impact of Reapportionment. By Timothy G. O'Rourke. (New Brunswick, N.J.: Transaction Books, 1980. Pp. xiv + 213. $14.95.)

The ink had scarcely dried on the Supreme Court's 1964 reapportionment decisions when several political scientists attempted to measure the impact of malapportionment prior to judicial entry into the political thicket. Employing a variety of statistical methods comparing state districting patterns with policy outcomes, these studies found little correlation between malapportionment and public policies unfavorable to urban areas or "liberal" causes. The implication, seized on by critics of the Warren Court's one-person-one-vote decisions, was that the reapportionment revolution had been waged to no avail because it would not produce the results widely anticipated. While these "skeptics" generally stated their conclusions with a high degree of certitude, one of them, Thomas Dye, had the prudence in 1965 to attach this caveat: "Quantification necessitates a simplification of what may be a very complex question. The consequences of reapportionment may be so subtle and diverse that they defy quantitative measurement. Perhaps the consequences in each state will vary so much that interstate comparisons are inappropriate" (*Journal of Politics* 27: 600).

In 1971, William Bicker (in Nelson Polsby, ed., *Reapportionment in the 1970s*, California) subjected these impact studies to a searching and penetrating analysis, concluding that they were flawed by a variety of methodological problems, and did not constitute tests of either the arguments by "reformers" or of the potential impact of the Court's reapportionment decisions. Bicker declared a mistrial. Moreover, subsequent studies began to find some effects of changing representative patterns, suggesting that institutional transformations may require time to trigger results.

Judiciously taking account of this extensive literature, particularly Bicker's insightful critique, Timothy G. O'Rourke has designed a sophisticated conceptual framework in *The Impact of Reapportionment.* This timely volume classifies potential effects into four categories. The first level embraces representative changes that result from any redistricting and that affect legislator-constituency relations, potential legislative turnover, etc. Second-level effects comprise the impact of reapportionment on group and partisan balance in the legislature, including urban/rural representation. Third-level effects include the impact of redistricting on legislative procedure, committee organization, and patterns of legislative conflict. Finally, fourth-level effects refer to the potential connection between reapportionment and policy outcomes.

O'Rourke then convincingly argues:

> In order for reapportionment to bring about measurable policy change, a series of developments rather than the single event of redistricting must transpire. First, reapportionment must lead to abnormally high turnover or, at the very least, enough district modification to alter legislative representation in a detectable fashion. To the degree that policy reflects group demands, redistricting must produce sufficient modification of group representation in the legislature (for example, the distribution of urban-rural or partisan interests) to make policy change likely or possible. If these first two conditions are met, measurable policy change would still require surmounting a variety of institutional barriers. Within the legislature, for example, stability in committee chairmanships and party leadership positions might mitigate major policy innovation despite substantially altered membership. In addition, legislative decision making must function within the context of previous decisions or laws and budgetary restraints which can limit policy innovation. It should be readily apparent that fourth-level effects, being several steps removed from the immediate impact of reapportionment, are likely to result only if reapportionment has produced sweeping changes in patterns of representation (p. 5).

O'Rourke then devotes a separate chapter to each of the four levels by analyzing patterns before and after reapportionment in the period 1962-1974 in six "sample" states: Oregon, South Dakota, Tennessee, Kansas, New Jersey, and Delaware. Why these particular states? The author does not really address this question squarely, except for a brief reference to differing or comparable patterns of urban/rural representation or party competition. Oregon, usually considered the best-apportioned state in 1962, appears to serve as a "control" case—i.e., since there was little actual malapportionment, no impact of consequence should follow reapportionment. Since O'Rourke had postulated (with supporting evidence from some studies) that the impact of reapportionment would be most severe in one-party states, it would have seemed appropriate for him to include in his sample a state such as Georgia, or Florida, or Alabama, where before 1962 urban areas were grossly under-represented in both legislative houses. The author does cite case studies in all three states revealing substantial impact of reapportionment. Still, O'Rourke's analysis, combining the depth of the case-study approach with the comparative elements of quantitative analysis, could have benefited from the inclusion of such a traditional one-party state. Augmenting the book's statistical data is a well-designed questionnaire sent to the 674 legislators of the six sample states in 1973, with a 43 percent response rate.

His investigations confirmed O'Rourke's initial expectations. First- and second-level effects were most immediate and discernible, while institutional patterns, established rules and customs, revenue limitations and past policy commitments tended to render third- and fourth-level effects more diffuse. Changes were least noticeable in Oregon and South Dakota, which had been well or fairly well apportioned prior to 1962. Impact appeared greater in states where changes in district patterns were most sweeping, replacing artificial non-metropolitan legislative majorities. But effects were also mitigated in states with strong two-party competition. Reapportionment helped increase the number of black legislators in Tennessee, Kansas, and New Jersey, and stimulated two-party competition for legislative control in Tennessee and Kansas. As for fourth-level (policy) effects, O'Rourke concludes: "The evidence presented suggests that reapportionment has affected selected policies in all sample states, but that the overall influence of reapportionment on policy has been rather limited and immeasureable" (p. 151). Among fourth-level effects were: more favorable distribution of state expenditures to central cities in most sample states; generally more "liberal" policies and possibly a more favorable climate for labor interests in all six states.

Prior to the 1960s a key indictment against malapportionment by "reformers" was that it enhanced divided party control in many states by virtually guaranteeing one party dominance of either or both legislative houses through the very pattern of districting plus partisan geography (perhaps most aptly captured by Governor Alfred E. Smith's lament in the 1920s that the New York legislature was "constitutionally Republican"). While there seems to be strong evidence that judicially prompted reapportionments have changed this condition in several states, O'Rourke does not develop this aspect well and even blurs it (see pp. 68, 157). In the case of New Jersey, he concedes that reapportionment "opened the possibility for Democratic control of the Senate" (p. 52), but then considers this to be offset by suburban Republican gains in the Assembly. But this approach tends to confuse the issue, which was not meaningful party competition but rather what the late V. O. Key, Jr. termed "the rigged rules of the game" (*American State Politics,* Knopf, 1956, p. 70).

On the whole, however, Timothy O'Rourke has made a most valuable contribution to our understanding of a complex issue. *The Impact of Reapportionment* reminds us that government decision making is intricate and full of nuances, not reducible to a simple stimulus/response mechanism.

GORDON E. BAKER

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Politics in Public Service Advertising on Television. By David L. Paletz, Roberta E. Pearson, and Donald L. Willis. (New York: Praeger, 1977. Pp. xi + 123. $14.95.)

Last spring quarter one of my students, an advertising major of reformist bent, agreed to do a term paper on the Advertising Council. Apparently not a diligent researcher, she found a number of references, and some of the self-promoting publications of the council. Under pressure to produce more, she interviewed some principals of major San Francisco advertising firms, and in the end produced a B-minus sort of thing.

What she didn't know, but should have known, was that David Paletz et al. had done the job three years ago. I didn't know it either, of course, and would have been still ignorant of this monograph had not the *APSR* book editor two months later asked for a rapid review of "a little volume that deserves professional notice."

Indeed it does deserve professional notice. Not that there's anything startling, especially insightful, or even surprising about *Politics in Public Service Advertising.* It isn't a book you can't put down—it has, at time, even a pedantic quality inducing yawns.

Its strength is that it describes clearly and in just the right amount of detail the fig-leaf of the advertising profession, the Advertising Council. Founded in 1942 jointly by the national government and business interests, named then the War Advertising Council, its mission was home-front propaganda. Since dropping "War" from its title in 1946 it has been promoting non-controversial public virtue for governmental agencies and for the more antiseptic private tax-exempt groups.

Among the associations for which the Advertising Council provides free or at-cost TV spots is Keep America Beautiful. Their ads are four-square against individual litterers, but omit all reference to industrial pollution of waterways and to petroleum-origin air pollution. Major supporters of Keep America Beautiful are Pepsi, American Can, and United States Brewers Association; KAB "has actively opposed legislation to require . . . returnable beer and soft drink containers" (p. 17).

On the other side, council screening rules—calling exclusively for national-scope, non-commercial-sales, nonpartisan, non-controversial and non-sectarian messages that "do not attack others"—keep out groups like Freedom From Hunger and Zero Population Growth. Especially illustrative is the effort of the Council on Children, Media and Merchandising to get aired their public service announcements on snack foods and the relationship between sugar consumption and dental problems. CCMM had the impertinence to ask the three TV networks to show their spots "twice each Saturday morning and once each weekday between 4:30 and 6:30 p.m." The networks declined the offer in favor of AMA and American Dairy Association spots which did not "take a negative approach."

The evidence of this research demonstrates that television's public virtue is bland, non-partisan, non-political, and that it appeals for "solutions" to problems by individual altruism within a consensual society. The fact that controversy is inevitable in public life, that governmental action is often necessary, and that private enterprise may be inimical to the public good—such blasphemies are never admitted by the Advertising Council. The Participation urged by the council is of three kinds: (1) solicitation ("Give to the college of your choice"), (2) recruitment ("Join the Marines"), and (3) personal admonitions ("Fasten your seat belt").

Palentz and his collaborators seem to think that public service announcements should be controversial and truly politicized, and that messages should be aired from groups other than those promoting patriotism and apple pie. They also assert that such announcements are effective—that though they appear not to change people's attitudes or behavior, they do "canalize predispositions and reinforce existing behavior." A late chapter in the book, "Including the Excluded," reports some free-lance and left-liberal successes in penetrating the public service announcement world without the Advertising Council's help—to the benefit of organized gays in California, post-Vietnam amnesty for war resisters, the United Farm Workers, and some anti-growth and small-is-beautiful groups. The authors' implications are clouded: if public service announcements don't change attitudes, why bother promoting gays, draft-dodgers, stoop-laborers, and progress-doubters?

Despite its stereotypical left-lib cliches, this is a useful piece of documentation on some hitherto unexplored features of television as business.

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Community Power and Political Theory: A Further Look at Problems of Evidence and Inference, Rev. ed. By Nelson W. Polsby. (New Haven: Yale University Press, 1980. Pp. xxvi + 245. $22.50, cloth; $5.95, paper.)

If you liked the first edition, you'll love the second. Readers expecting a conciliatory, centrist, mellowed synthesis of a major intellectual controversy will be disappointed. Those anticipating a challenging, argumentative, feisty rendition will be more than satisfied. For now Nelson Polsby, having dissected the stratificationists in his first outing, strikes back at the critics of the pluralist approach in general and the New Haven study in particular in this much-expanded new edition.

Although the new book has thematic integrity throughout, the old and new parts do constitute different segments in the history of the study of community power. To help set the additional installment in place, I will quote at some length from a review I wrote of the *first* edition in 1964 (*Journal of Politics* 26: 939-40).

"Polsby's basic objective is to explore (and explode) what he calls the 'stratification theory' of community power. Five key propositions of this theory are posited. . . . The author questions the validity of these propositions by examining in detail the major studies of community power which seem to have been guided by stratification theory. He goes on to 'test' the theory by laying it against the New Haven data. . . . Essays on why the theory is so prevalent, the pluralist alternative to studying power, and what is required for a theory of community power round out the book.

"The principal merit of this *tour de force* is that it juxtaposes the theoretical underpinning of the two research approaches (stratification and pluralist) and their attendant empirical results. It is difficult, in view of his thorough documentation, to reject Polsby's contention that the stratification writers have failed to match theory and results. His argument that the pluralists have been more successful in this regard is less well documented, though still persuasive."

I went on to hazard that "the book will occasion some controversy"(!) and concluded that "this is a work of extreme relevance to those interested in the study of community power, the relationship of theory to research, and the sociology of knowledge." I am reasonably content to stand by this appraisal, though I was not sufficiently prescient to anticipate the staying power of the book.

Why did the initial work succeed so well? The answers are important because they bear on an evaluation of this lengthy addendum. First, Polsby is a master at distilling and clarifying. He has the touch, the instinct for divining and setting in place contrsting points of view. Second, he is thorough in his scholarship; he cannot be accused of being selective in his source materials (though some will quarrel with him on his *interpretation* of these materials). Third, he marshals an argument exceedingly well. This is the reason the book has so often been the focal point of the anti-pluralists. Fourth, Polsby writes in a clear, often pungent fashion. His prose is laced with asides and irreverencies that can only delight the reader.

The virtues of the first effort are present in full force in the augmentation, which consists of a reflective preface and six new chapters. Polsby first takes stock of his earlier contribution by concluding, through quoting their own work, that the stratificationists have basically accepted his critique of their failure to have demonstrated empirically the presence of cohesive ruling elites in American communities. He then goes on to address several specific charges brought against pluralists and the New Haven study. The questions and answers can be put as follows. (1) Do pluralists, as well as stratificationists, fall victim to their own theoretical and methodological orientations? No, and assertions to the contrary remain asertions until backed by evidence. (2) To what extent were the original findings and interpretations of the widely cited New Haven study in error? Specific charges, claims, and misstatements about weaknesses in the New Haven study can be refuted by textual references (shades of inerrancy here). (3) Is the second face of power, that of nondecision making and the mobilization of bias, hidden from the misdirected tools of pluralist researchers? No, whereas the first face persists largely without blemish, the second face, though occasionally glimpsed, remains largely one of imagery and conjecture. (4) Do pluralists err in not distinguishing more clearly between the manifested preferences and the real interests of political actors and do they not overlook the problems of enlightened choices and false consciousness? Well, the subject of interests and preferences is troublesome, indeed, but pluralist researchers have not ignored actors' interests to the extent of painting a false picture of community power.

Of these new chapters the one dealing with the mobilization of bias strikes me as the most powerful and generally most useful one to a variety of readers. This theme is perhaps the most tantalizing and intuitively appealing critique made against the pluralists. Yet point by point Polsby neutralizes the criticisms, though ultimately conceding that if non-decisions could be studied successfully this might enhance our understanding of community power. Much of this chapter is vintage Polsby, his rapier in full play.

Just as I was beginning to compose this review I received a notice that Floyd Hunter's new book on Atlanta was imminent. Having read the manuscript version, I can assure the world that community stratification theory is alive, though whether it is also well is more problematic. So the eighties should not lack for a little excitement. And the reason is not a consuming interest in local politics per se. I agree with Polsby that "as a source of cumulative knowledge about the governance of local communities, the community power literature is disappointing. As a smallish arena for the ventilation of some of the central metamethodological issues of social research, this literature has some didactic value" (p. xvii). With this fresh installment, he has done his part to keep the action alive in that arena.

M. KENT JENNINGS

University of Michigan

Urban Policy and the Exterior City: Federal, State and Corporate Impacts upon Major Cities. By H. V. Savitch. (New York: Pergamon Press, 1979. Pp. xv + 359. $35.00.)

Books which describe the decay of major American cities, catalog the urban programs which are intended as responses to that decay, and cite the failures of states and nations to develop an effective urban policy are not unusual. However, the approach taken in *Urban Policy and the Exterior City* is unusual. H. V. Savitch argues that urban decay is a problem of the "exterior" city and that forces external to the city, i.e., national authority, middle governments, and private power, have created the circumstances which have led to its destruction.

The key to Savitch's argument is the concept of privatism, by which he means "the larger corporate structure in American capitalism and the ability of its individual weight . . . to make a difference in the size and composition of cities" (p. 3). National authority, e.g., presidents and their administrations, and middle governments can be classified either as reinforcing this privatism or as adopting "meliorist" policies which attempt to intervene on behalf of the cities. Savitch discusses recent national administrations and the states in these terms and concludes that, to varying degrees, Kennedy, Johnson, California, Maryland, Massachusetts, and New York are "meliorist" while Nixon, Ford, Louisiana, Missouri, and Ohio are "reinforcing."

There is considerable virtue in challenging the conventional wisdom which focuses on the political and administrative failures of city governments as contributors to urban decay. However, Savitch's attempts to create clear, dichotomous classifications are not fully successful. At the outset, sunbelt cities are excluded from the exterior city category because they have not yet experienced urban decay (p. 1). There are serious logical problems with such an exclusion. Privatism and intervening national and middle government policies clearly have an impact on cities which are classified as "most advantaged" as well as on those that are "disadvantaged" (p. 37). The impact is simply different. The exterior city concept is a useful one, but it is a serious mistake to apply it only to the decaying, disadvantaged city. One can no more understand Houston by examining only "interior" factors than one can understand Cleveland in this way. Privatism, often reinforced by national, state, and local policies, is tremendously advantageous to some cities (if physical and economic growth is always an advantage) and disastrous to others. The model of the exterior city and the assumptions underlying it all are ill defined.

The classification of national and state policies as "reinforcing" or "meliorist" is overly simple. Savitch finds himself uncomfortable with his own categories in that he sees Kennedy as less meliorist than Johnson (p. 111) and finds several states to be "mixed or ambiguous" as to type (p. 247), to cite but two examples. The labels probably can best be seen as two points on a continuum. By implication, there is something beyond meliorism which would deal more effectively with privatism (or even eliminate it), and it is possible to go beyond reinforcement. The criteria used by Savitch to place administrations and states on the continuum are not clear. In the case of states, they are tied to state aid to selected cities and to differences between state aid for welfare and state aid for highways (p. 246). However, because the analysis is limited to 10 states with exterior cities, our reference points are muddled. Are Arizona or Texas (not included in the analysis) more reinforcing of privatism than Ohio? Is Ohio closer to New York on the scale than it is to Arizona? Is Ohio, then, really reinforcing rather than meliorist? The data are incomplete, and we really have no answers.

Urban Policy and the Exterior City is a fundamentally discouraging book. It begins with the assertion that "what corporations and the shakers of the economy do determines what Washington and middle governments will do" (p. 12); rejects many recent national efforts as "policies [which] may meliorate the festering wounds of the city and the difficulties of chronic unemployment, but . . . do nothing basic to solve these problems" (p. 6); and rejects "the commonly held belief that Washington has already done what it could for cities and is not to blame for their failure" (p. 6). By implication, we can conclude that Washington should do more, that "more" does not mean additional meliorative policies, and that the determinate relationship between privatism and public policy should be broken. Savitch does not take the next step and offer such a prescription. If his first assertion is correct and corporate decisions do determine national, state, and local governmental policy, then the future is indeed bleak, given his analysis. If the relationship is not wholly deterministic, then Washington and the states have some hope of reversing the tide.

There is evidence that Savitch has overstated his case and that governments can and do affect private economic decisions in a significant manner. State and federal environmental protection regulations influence corporate decisions with respect to plant locations and the existence of federal minima guarantees that corporations are restrained to some degree from playing one state against another. While federal and state efforts have been modest, they do provide evidence that Savitch's "determines" is too strong a word and that there is some hope for those who view government intervention in the allocation of private resources among urban areas as a major part of the solution to problems of urban decay.

Savitch's analysis is too simplistic. It is crucial to recognize the role of private, corporate decisions in urban policy, but the web of interrelationships is considerably more complex than the one which he suggests.

JOHN H. BAKER

Southern Illinois University, Carbondale

The Future of U.S. Politics in an Age of Economic Limits. By Bruce M. Shefrin. (Boulder, Colo.: Westview Press, 1980. Pp. xii + 251. $24.50.)

This book considers future U.S. political and economic systems based upon a slow-growth or no-growth American economy. Bruce Shefrin argues that the existing U.S. political system—consensus politics—reduces political conflict mainly through the lower and middle classes' expectation of progress based on their belief in economic growth, i.e., greater opportunity and prosperity. He indicates that consensus politics has limited redistributive policies, has moderated class consciousness, and has maintained economic inequality among classes.

Shefrin asks: if consensus politics is weakened by a decline in economic growth, then what changes will occur in American political attitudes and behavior and the conflict-resolving mechanisms of the political system? He suggests that lower levels of economic growth will lead to an increased perception of economic inequality and more intensive types of political and economic conflicts than are found under consensus politics. A limitation in this book is that Shefrin does not indicate the process through which these attitudes will change, nor does he show that these types of attitudes changed during earlier periods of U.S. economic decline.

Shefrin accepts the likelihood that the U.S. economy will have limited growth in the near future. His position is consistent with the views of D. Meadows et al. *Limits to Growth* (Universe, 1972). Shefrin does not examine either the criticisms of Meadows' analysis or the revised economic model in M. Mesarovic and E. Pestel, *Mankind at the Turning Point: The Second Report to the Club of Rome* (Dutton, 1974). These omissions reflect shortcomings in Shefrin's analysis of physical limits to growth—raw materials and industrial effluents—and his assumptions about a future based upon limited growth.

Shefrin offers a speculative analysis of four scenarios in a future of limited growth. Each scenario is based upon a different ideological foundation—liberalism, neo-Marxism, corporate-conservatism, and traditional conservatism. He argues that there are different combinations of political and economic systems. These new structures depend upon a serious challenge to consensus politics leading to social awareness of slow-growth or no-growth economy. Shefrin's emphasis on the ideological polarization underlying political and economic systems adds an important element to limits-to-growth analysis. However, further conceptualization of political change is needed to explain how the new awareness will occur under the alternative political and economic scenarios.

The study of changes in a society's political behavior and attitudes during periods of economic instability or economic decline is an important question for political scientists. Shefrin raises thoughtful questions about the relationships between attitudes toward the economy and political behavior. His study leaves room for further conceptual and empirical analysis of those questions and about future U.S. politics.

STEVEN PURO

St. Louis University

The Failure of the Franklin National Bank: Challenge to the International Banking System. By Joan Edelman Spero. (New York: Columbia University Press, 1980. Pp. xi + 235. $14.95.)

Joan Spero has painstakingly documented the most dramatic episode in recent U.S. banking history: the collapse of the Franklin National Bank. Franklin held $5 billion in assets and was the twentieth largest U.S. bank before it began its slide. The failure of a bank as large as Franklin would be a serious matter any time, but was especially so in 1974, in the context of a worldwide recession, high inflation, uncertain prospects for petrodollar recycling, large fluctuations in exchange rates, and announcements of other banking problems, including closure of Bankhaus Herstatt in Germany while efforts were under way to salvage Franklin.

Spero's thesis is that: (1) banks, especially U.S. banks, rapidly expanded their international operations after 1960, inevitably attracting some weak, ill-prepared participants like Franklin; (2) international banking involves different, possibly larger risks, particularly hazardous to newcomers; (3) the international banking system is highly interdependent because it relies so heavily upon interbank transactions, including loans, deposits and foreign exchange trading; (4) the system is fragile because a deterioration at one bank can leave others vulnerable as well; and (5) the absence of an international lender of last resort, together with gaps in bank supervision, suggest that the system may be unable to prevent or manage future crises. Improvements were made after 1974, but Spero believes stronger bank supervision and clearer demarcation of responsibilities may be needed.

Spero's account is excellent history—important reading for those interested in money and banking or in international economic relations. The policy implications could be expanded, however.

The United States has developed one of the most extensive and restrictive bank regulatory and supervisory systems in the world. U.S. banks are not "national"—their operations in the U.S. are generally restricted by law to a single state, sometimes to a single location. U.S. banks are barred from many lines of business in which foreign banks engage. U.S. banks are subject to frequent audits and examinations by federal and state authorities.

One of the few growth opportunities for U.S. banks lies in international banking. Franklin took the plunge and failed, but there is little to support the view that Franklin failed because of its international activities. Spero notes that Franklin was weak on virtually every count: "weak management, a bad domestic loan portfolio, poor investments, heavy reliance on very short-term loans" (p. 11). Excessive foreign exchange speculation, illegally concealed, helped bring down the bank, but it was undertaken in a futile effort to make up losses on domestic, not international, operations.

Surprisingly, the extensive American system of bank supervision and regulation failed to prevent Franklin's collapse, and it took the three federal agencies concerned (the Federal Reserve Board, FDIC, and Comptroller of the Currency) five months to resolve the crisis. International cooperation, on the other hand, was smooth and effective in the Franklin case. Perhaps improvements in the international banking system should begin at home?

ROBERT W. RUSSELL

U.S. Senate Committee on Banking, Housing and Urban Affairs

Rolling Thunder: Understanding Policy and Program Failure. By James Clay Thompson. (Chapel Hill: University of North Carolina Press, 1980. Pp. xv + 199. $14.00, cloth; $6.50, paper.)

"Rolling Thunder" was the code word for the sustained bombing campaign carried out by the United States against North Vietnam between February 1965, and March 1968. The campaign began as a reprisal in connection with the Pleiku incident. Shortly it became a long-range program with a multiplicity of objectives, to include forcing the North Vietnamese into serious negotiations. It was ended by LBJ in his "abdication" speech of March 31, 1968, in the aftermath of the Tet Offensive.

There seems little doubt that the campaign was a strategic failure, and Thompson is quite clear on this. He uses the campaign as a case study of the workings of the American national security apparatus. In particular, he attempts to determine why it took so long to discover that Rolling Thunder was not accomplishing its objectives and why the foreign/defense bureaucracy failed to adapt to the realities of that failure.

Thompson's approach is to begin with three descriptive chapters on the origins and failure of Rolling Thunder. This is followed by a chapter giving his interpretation of the evolution of organizational theory, especially that of decision making. This chapter, by the way, would be better placed as an appendix. The final chapter focuses on decision masking in the area of foreign and defense policy with a view toward interpreting the course and outcome of the Rolling Thunder campaign previously described.

Rolling Thunder was enormously expensive in material and human resources; the subject is therefore an important one. The standard air force explanation for its failure is that air power was applied incrementally as contrasted to being applied massively on such critical targets as Hanoi and Haiphong. Thus, this explanation goes, when Nixon finally applied massive air power late in the war, i.e., the Christmas bombing of 1972, the North Vietnamese agreed to the accords of January 1973. What this explanation overlooks is that the January 1973, agreement is, with minor differences, the same one which the U.S. could have received the previous fall. Certainly the North Vietnamese were happy to get the heavy bombardment over, as was the U.S. military, for the costs in aircraft and personnel were heavy. The question is, did the Christmas bombing really have any political utility in terms of the final settlement? Many, myself included, would say no.

In this context what is the value of Thompson's work? Certainly it is an excellent and concise description and analysis of the bombing campaign and on that level a very worthwhile effort. The author goes beyond that, however, and tries to determine why the U.S. national security apparatus failed to adapt to the realities of the environment, which clearly was not being influenced by the massive tonnage being delivered on North Vietnam. His answer, though not highly original, is interesting. He concludes that Rolling Thunder was shaped more by "the internal needs of the [U.S. national security] bureaucracy, than to the problem at hand." Further, says Thompson, the program resembled more an "anarchy" than a coherent military program designed to achieve foreign policy objectives.

While this work is probably not the final word on the strategic failure of the American bombing campaign in North Vietnam, it is more insightful than the simplistic post hoc explanations of the U.S. Air Force. Overall, the case seems to be another example of foreign/defense policy decision making being influenced more by process than rational analysis.

DOUGLAS KINNARD

University of Vermont

Policy Implementation in the Federal System: National Goals and Local Implementors. By Carl E. Van Horn. (Lexington, Mass.: Lexington Books, D.C. Heath, 1979. Pp. xiv + 178. $18.95.)

The legislative successes of the Nixon administration's New Federalism proposals raised several questions regarding the potential change in intergovernmental relations. How are national goals reconciled with often conflicting local priorities? What happens when the implementing rules are changed from nationally controlled categorical programs to more loosely structured block grants? How does the political process change and who benefits from such changes? Carl Van Horn combines two areas of study for political scientists, public policy and intergovernmental relations, to address these questions. Taking the General Revenue Sharing program (GRS), the Comprehensive Employment and Training Act (CETA), and the Community Development Block Grant (CDBG), Van Horn develops and applies a model of intergovernmental policy implementation to analyze what transpires in the implementation process of these three important cornerstones of the New Federalism.

Van Horn's model identifies three variable clusters as a means to analyze the programs. The first, policy standards and resources, examines the law (its priorities, procedures and prohibitions, and its contradictions and ambiguities) and its resources (funding availability and timing, and contribution to recipient communities' budgets). The second, the national policy environment, explores the influence of national organizations, and national-level administrators and elected officials who attempt to influence policy implementation. The third, the local policy environment, investigates the attitudes and influence of various local actors, the social, economic and fiscal conditions of those areas and the characteristics of the agency responsible for carrying out the law.

Van Horn's book confirms the intergovernmental changes brought about by the implementation of the New Federalism policies. In both the CETA and CDBG programs, locally elected officials became more involved in implementation decisions than they were under the more centralized period of the categorical programs. Conversely, GRS produced no significant changes from normal (pre-GRS) decision-making practices at the local level. While the New Federalism programs initially reduced the influence of national administrative agencies in the design and implementation of the laws, these agencies have become more aggressive in their roles through more assertive enforcement of procedural requirements and providing direction in the substance of local programs. This is evidenced by a greater mandate for monitoring and evaluations in the reauthorization of the programs. In the use of funds under these programs, spending in the CETA program seemed to be the exception, in that minimal program reform was achieved with the changes from the categorical to the block grants. GRS funds were spent primarily on capital projects with a minimum amount allocated to social service programs. In contrast to patterns established with slum clearance, urban renewal, and social services offered with Model Cities programs, communities spent CDBG bunds increasingly on smaller, short-term projects directed toward public works and housing conservation and rehabilitation. The beneficiaries of the three programs were as varied as the programs themselves. As GRS was not designed to benefit low-income people, few such direct benefits were so distributed. The inquiry into benefits attributable to the CETA and CDBG programs produced mixed results. Combining the summaries of analysis from each program, Van Horn offers the generalization that some amount of flexibility or lack of legislative clarity may be necessary for effective implementation—though to insure redistributive outcomes, it appears necessary to establish clear national goals.

This book is an interesting mix of reporting the findings from several studies (particularly on GRS) and Van Horn's own work, yet it is not without certain weaknesses. At times, the reading of the book slows, perhaps because of the difficulty in integrating those studies through a new and differently focused lens. While CETA and CDBG programs provide an opportunity to compare the consolidated grants with their predecessor categorical counterparts, there is some difficulty in including General Revenue Sharing. GRS is a significant component of the New Federalism, yet it does not have a nationally based program as a predecessor for comparison. The book claims a readership of those interested in the study of public policy, intergovernmental relations, urban politics and/or implementation and certainly such a wide audience will find at least parts of the book directly related to their interests. However, as Van Horn warns at the outset, this book does not provide specific details about each program. Consultation with the well-referenced case studies is appropriate. The author also acknowledges the major weakness of the model: that the variable clusters are not as comprehensive as they might be. He also restricts his investigation by looking only at selective parts of each program (among them, enforcement of non-discrimination provisions of GRS and the distribution of public service jobs in CETA). Nonetheless, the rigor employed in the analysis helps explain the complexities of program implementation and the changes which have occurred through the New Federalism efforts of the early seventies. For political scientists interested in a systematic overview of those efforts, this book provides a very good opportunity to assess the factors influencing the implementation of different programs by employing a useful model. While that model is not as comprehensive as it could be, the effort is commendable as the book attempts to find the seldom-reached middle ground between the various frameworks for studying public policy and the plethora of case studies which abound.

DOUGLAS H. SHUMAVON

Miami University

Unloving Care: The Nursing Home Tragedy. By Bruce C. Vladeck. (New York: Basic Books, 1980. Pp. xiii + 305. $13.95.)

In the introduction to this Twentieth Century Fund Study, the author is described as a "young and untested scholar interested in public policy issues . . . who had a grasp of the complexity of his subject and cared passionately about the shortcomings he discovered" (p. ix). Bruce Vladeck is a health planner for the New Jersey Department of Health in addition to teaching public health and political science at Columbia. He is qualified to deal with this complex, dreary, and increasingly important facet of public policy. Although the text could have been better organized, Vladeck reveals a clear understanding of the goal of nursing home care: "High quality nursing home care consists of the maintenance of a clean and pleasant environment, in which the food is good, . . . there is plenty to do, assistance is readily given with dressing and bathing, people are nice to each other and respect each other's privacy and personal dignity, and good medical and nursing services are provided to those who need them" (pp. 148-49).

He also reminds us that nursing home care in the United States is a public problem: 57 percent of nursing home income comes from the public purse and almost 90 percent of that amount comes via Medicaid, the federal welfare program for the aged and medically indigent.

Vladeck provides just enough statistics to demonstrate the intractability of the nursing home problem. Although only one in ten citizens over 75 lives in a nursing home, the average age of the institutionalized elderly is over 80. More than half cannot walk unassisted; 30 percent are incontinent. White females over 80 constitute the modal group of nursing home residents.

Vladeck's description of the history of nursing homes and government policy initiatives constitute the middle and longest portion of the book. Vladeck shows a sophisticated understanding of the political process as it related to hospitals and nursing homes in the 1970s.

I found the prescriptive portion of *Unloving Care* disappointing, however, perhaps because I expected too much. Vladeck's claim that private needs can be met only by public action (p. 266) isn't persuasive, particularly with regard to problems of the aging which are treated largely outside the public domain by the American middle class and even more so within the black and brown ethnic communities. The author recommends federally mandated reductions in the number of nursing home beds with the residual to be reserved for short stays for the approximately 150,000 aged who are terminally ill and those needing interim care, plus the 600,000 who require round-the-clock custodial care (p. 223). Standards would be maintained by a complex set of oversight procedures including inspection, peer review, and public scrutiny.

Certainly the current policy schema of incentives and reimbursement by level of care hasn't produced quality nursing care, particularly in homes populated largely by Medicaid recipients. But, implementation of Vladeck's policy seems unlikely to produce more loving care than the policies currently in force; I believe that increased regulation would promote consolidation of nursing homes and would lead inexorably to the depersonalization of services to aged residents which Vladeck criticizes.

Sadly, Vladeck did not attend carefully enough to some of the newer approaches to care for the aged or to the ways government might encourage these new approaches. He discusses home care only briefly (pp. 215-16) and mentions the hospice movement not at all. He fails to examine the ways in which some local governments are combining federal programs, such as senior luncheon activities and the subsidized housing program, to provide creative and humane options for the ambulant elderly.

As for the aged and infirm, careful reading of *Unloving Care* leads me to the unappealing conclusion that there is little help for the lonely old lady trapped in a filthy and understaffed nursing home with nowhere else to go.

GAYLE AVANT

Baylor University

Legislated Learning: The Bureaucratization of the American Classroom. By Arthur E. Wise. (Berkeley: University of California Press, 1979. Pp. xvi + 219. $10.95.)

Criticism of the extraordinary growth of government regulation of education has been an insistent theme of academic professionals in recent years. *Legislated Learning* is an analysis of this problem that makes an important contribution to the literature. Part of the book's significance is Arthur Wise's particular background. He is a former officer of the National Institute for Education and is currently a senior social scientist with the Rand Corporation. More relevant, it was his legal analysis in *Rich Schools, Poor Schools* (Chicago, 1969) that intellectually launched the modern movement to equalize public school finance. Although the U.S. Supreme Court in a 5-4 decision (*San Antonio* v. *Rodriguez*) rejected Wise's constitutional argument, many state courts accepted his thesis. As these courts set about reforming their state's educational fiscal systems, they inadvertently had the effect of strengthening state dominance of local school boards and increasing educational bureaucracy in general. This occurred most dramatically in New Jersey where the state Supreme Court in six different opinions (called *Robinson* v. *Cahill* I through VI) moved from a concern with equalizing school finance to a complete revision of the goals and structure of the educational system. The court frequently cited *Rich Schools, Poor Schools* with approval and in *Legislated Learning* Wise devotes a chapter to detailing the policy evolution in these decisions.

Given his background, then, it comes as a bit of a surprise that Wise's new book is a critique of too much bureaucratic and judicial regulation. He, of course, does not reject all regulation. He tries instead to make a distinction between regulation to achieve equality which he believes is necessary, and regulation to insure efficiency and effectiveness—most of which he thinks is misguided. The distinction, however, as he concedes, is increasingly difficult to sustain.

There is a drive for accountability apart from any concern for equity. Wise identifies some 11 different management concepts or tools that have been applied to education, ranging from competency-based education to performance contracting. By 1974, there were at least 73 laws enacted by states mandating educational "accountability."

Increasingly, however, the search for accountability and the search for equity have intertwined. As the goal of equal *opportunity* in the sixties evolved into a demand for equal *results* in the seventies, legislatures and courts discovered that the new form of equality could not be achieved without regulating all of the contributors to the efficiency/effectiveness of the entire school system. Nor is Wise's distinction entirely plausible that equity regulation is political and therefore within the knowledge of courts and legislatures, while intervention for efficiency/effectiveness is technical and beyond the capacity of courts and legislatures. Equalitarian regulation in the form of affirmative action plans, challenges to academic testing and employment selection or judicial supervision of school integration often have substantial technical components. It is not apparent that courts and legislatures know how to create equality any more than they know how to create efficiency/effectiveness.

But, if Wise's criticism of the source of regulation is a bit partial (perhaps for liberal ideological reasons), his analysis of the consequences of the growing school bureaucratization is very persuasive. He describes a number of trends, increased litigation, assertive legislatures, ambitious teacher organizations and powerful single-issue groups, which contribute to forcing educational policy to higher and higher levels of the political process. Since these levels can only deal with schools, teachers and students as abstractions, the result is "hyperrationalization" (Wise's favorite term) of educational policy.

"Hyperrationalization" is the attempt to establish rules and procedures for all elements of the educational process and to specify all goals in ways that have measurable outcomes. This may make schools less arbitrary and educational results more predictable; but it reduces teacher discretion and incentive, the variety and competition among schools, and leads to a reductionist view of the purposes of education. Being the easiest data to gather, credit hours, student-teacher ratios, standardized test scores come to dominate discussions of educational policy. Students are seen as products subject to cost and quality control rather than as full human beings.

What is the likely future of this development? Are there alternatives? In his final chapter, Wise suggests four possibilities. The most likely future development is incremental bureaucratic centralization, but the other possibilities are "planned bureaucratic centralization, responsible local control, the rationalistic solution and the rational solution." Wise notes that some might conclude that it would be better to plan bureaucratic centralization than to have it occur incrementally. He suggests that centralized educational systems elsewhere in the world and large-city school systems in the United States be studied as a prerequisite for such a step, but Wise is not enthusiastic. Nor does he think local control will work, given the continuing need to assure equality. The rationalistic solution which begins with the premise that education needs improved policy tools, principally productivity-improving devices, does not have a record of success, according to Wise. That leaves "the rational solution," which Wise defines as maintaining regulation for equality but recognizing that we do not know enough to regulate for increased productivity. Wise prefers this approach and he brushes aside vouchers and school decentralization in a few paragraphs. But the rational solution concept is never really developed and it is hardly a "solution" in any event. In the meantime, regulation of education by many agencies having many motives will continue to grow. Wise has given us a powerful statement of the costs of that trend, but sees no solution.

GEORGE R. LANOUE

University of Maryland, Baltimore County

Who Votes? By Raymond E. Wolfinger and Steven J. Rosenstone. (New Haven: Yale University Press, 1980. Pp. x + 158. $15.00, cloth; $3.95, paper.)

This study examines the impact of registration procedures and 17 demographic variables on the probability of voting in presidential and congressional elections. The Census Bureau's Current Population Surveys for November, 1972 and 1974, were the main sources of data, and probit estimation was the principal mode of analysis. The authors report their findings with unusual care and clarity.

The huge census samples (of over 90,000 respondents) permit exploration of the demography of voting and nonvoting in rich detail. The authors nicely disentangle the separate effects of education, income, and occupation and, holding other factors constant, show also that:

- the probability of voting increases with age up to and through the seventies;
- housewives are as likely to vote as working women;
- marriage increases the probability of voting, with the effect weakest among the young;
- the turnout of governmental employees in "patronage" states is significantly higher than their turnout elsewhere;
- Chicanos are as likely to vote as other voters, Puerto Ricans are not.

Other findings of interest abound.

The census samples are also very helpful in isolating the effects of various registration procedures and are well exploited to that end. One has greater confidence in the study's positive findings in this regard, however, than in some negative ones. Laws authorizing neighborhood registration *may* make no difference, but their apparent insignificance could also reflect inability to identify variations in the implementation of such laws. Similarly, while *provision* for the use of deputy registrars does not increase voting, their actual use may, as the authors note.

The census surveys make no effort to determine whether respondents feel obligated to vote, prefer a candidate, think voting accomplishes something, find the campaign interesting, see the race as close, or have partisan affiliations. The absence of these and other psychological variables from the authors' data limits not only their ability to explain some of the variance in turnout but also their ability to trace the connections between demographic variables and rates of voting. For instance, while they show convincingly that education has a far more powerful and pervasive relationship to voting than income or occupation, they cannot, given their data, say why. Their speculations on the matter are sensible, but speculation is not explanation.

The study concludes with this statement: "For the present, it appears that advocates of both sides of major controversial policy issues are represented among voters in proportion to their numbers in the population" (p. 114). This finding is well supported, and the analysis that produced it is an obviously good move in assessing the consequences of differential turnout among population groups. The finding is sure to suggest to some that differential turnout is of no importance, however, though the authors do not draw that conclusion. Properly. Unless one can show that full-turnout elections would have no impact on the strategies of candidates, one cannot be sure that the issues now raised are the ones they would raise in the absence of group differences in turnout.

In the tasks it sets itself, I do not expect soon to see a book that surpasses the achievements of this one.

STANLEY KELLEY, JR.

Princeton University

Comparative Politics

Religion and Politics in Contemporary Iran: Clergy-State Relations in the Pahlavi Period. By Shahrough Akhavi. (Albany: State University of New York Press, 1980. Pp. xix + 225. $29.00, cloth; $9.95, paper.)

Shahrough Akhavi has produced a book on a subject as tricky as it is timely, a book which is a triumph of intelligence and common sense, and the best guide that now exists to the role of the *ulama* (clergy) in twentieth-century Iranian politics. Although the style is sometimes dense, and reading it is occasionally hard going, especially in the introductory chapter, the later chapters, appendices, and notes repay close attention, as they include a great amount of information coupled with sensible and convincing interpretations. The book covers briefly the leading role of the *ulama* in the rebellious and revolutionary movements of 1890-1911; the diminution of their power under Reza Shah (1925-41) who stripped them of many legal, educational, political, and economic prerogatives; and especially their revived political role after 1941. The religious opposition of the Mossadeq period, represented chiefly by Ayatollah Kashani and the terrorist Feda'iyan-e Islam, are well covered, and there are facts not easily obtainable about the activities and writings of recent oppositional *ulama,* including Ayatollahs Khomeini, Taleqani, and Shari'atmadari. Also covered is the lay (not theologically educated) religious opposition. Particularly interesting to me is the discussion of anticlerical implications of some of the talks of the most influential man in this opposition, Ali Shari'ati, which were echoed by the cool reception given Shari'ati before his death in 1977 by all the leading ayatollahs. It is only since his death that many of the latter have tried to appropriate him. Throughout, the most important Persian and Western writings have been consulted by Akhavi.

Akhavi's book includes discussions of the economic and political sides of growing clerical discontent; depriving the *ulama* of positions meant depriving them of wealth and power. He notes that the Pahlavi rulers never gave significant attention to even the justified grievances of the *ulama* or their allies in the bazaar economy, underestimating the importance of both in their forced march to Westernization.

Although small errors may be found, such as the author's following Algar's account of Khomeini's deportation, which has anachronisms including dating it in 1963 instead of 1964, or writing *fiq* for the standard *fiqh,* Akhavi's account is generally reliable and, because it was largely done before materials on his subject were widely available, it is a remarkable and highly useful achievement.

Nikki R. Keddie

University of California, Los Angeles

The Politics of Economic Decline: Economic Management and Political Behaviour in Britain since 1964. By James E. Alt. (New York: Cambridge University Press, 1979. Pp. viii + 296. $24.95.)

This study of the intricate relationships in contemporary Britain among personal well-being, the state of the economy, and political outlook is by far the most thoughtful and exhaustive investigation on the subject. James Alt offers a useful explanation for why these troubled economic times have produced a politics characterized by cynicism and conservatism rather than a politics of activism and radicalism: that individuals have become more realistic about the condition of the economy and more pessimistic about the limits of government. Alt begins his investigation by acknowledging his debt to Butler's and Stokes' work of a decade ago. He does, however, reject Stokes' distinction between valence and positional issues, and argues that in recent years at least, every issue contains both valence and positional components. For example, everyone is opposed to unemployment, the valence component; but if full employment is linked in the individual's understanding to continued high inflation, then the positional component of the issue becomes apparent.

This interweaving of positional and valence dimensions is important to understanding Alt's central argument that Britons have grown in economic sophistication as economic troubles persist. Britons, Alt finds, clearly distinguish between their own individual economic prospects and their appraisal of the national economy. They tend accurately to assess national economic performance and increasingly to see the interconnectedness of economic issues. Their skepticism has grown over the abilities of successive British governments to improve either the voter's personal well-being or the condition of the nation's economy.

Alt's findings concerning the welfare state's future in a time of diminishing economic expectations will generate some controversy. Alt interprets support for the welfare state, particularly in those areas that extend benefits to relatively small subsets of the population, as altruism born of prosperity. An example is support for state aid to the unemployed, which for many Britons is an altruistic act, since few favoring state aid can personally expect to benefit from it. As personal economic conditions deteriorate, altruism diminishes, presumably because people increasingly notice and resent that their tax outlay is being spent to benefit others. Thus bad times, paradoxically, may hurt the Labour party because of its commitment to welfare-state expansion. Working-class Labour supporters experiencing bad times become more critical of increased welfare expenditures, leaving Labour's middle class as the only major group advocating increased welfare expenditure in a time of economic decline.

The limits to altruism argument is fascinating but requires further testing. Alt relies on British-sponsored SSRC surveys from 1964 to 1974 and commercial polls from that period, with a few drawn from the post-1974 period. A fuller exploration of the 1974-79 Labour government would prove useful. The real testing ground for Alt's thesis may well prove to be the Conservative government elected in 1979, after the book was written. The Conservative commitment to a sustained reduction in expenditure may place the welfare issue in sharper focus. Alt's examination of continued and deepening economic decline could have been further tested by offering more in the way of regional analysis that the book does. Alt only briefly mentions Scotland and rarely contrasts the much more economically troubled north—whose economy has been in decline for most of the century—with the fairly prosperous south fas a means of assessing the long-term effect of economic regional decline.

Greater attention to regional analysis or extending the study to future governments is needed to see if the altruistic-non-altruistic dichotomy is sufficiently supple to interpret attitudes in times of sustained deterioration and substantial challenge to the welfare state. For example, on the question of attitudes towards welfare-state expenditure, voters may support aid to specific groups on prudential rather than altruistic grounds either as a means of responding to group discontent or because they see for themselves some future need for the aid in question. Distinguishing between altruistic and nonaltruistic attitudes may prove to be as elusive as distinguishing between valence and positional issues.

These concerns, however, do not detract from what is a needed and valuable contribution to understanding political attitudes in an age of lowered expectations.

John G. Francis

University of Utah

Political Action: Mass Participation in Five Western Democracies. By Samuel H. Barnes, Max Kaase and Associates. (Beverly Hills, Calif.: Sage Publications, 1979. Pp. 609. $25.00.)

The longest-running epic since *Gone With the Wind* has at last appeared in print! Written over a period of 10 years by a collective made up of Europeans and Americans, it forms one of the most extensive and sustained pieces of comparative research yet produced in political science. It is indeed resolutely comparative, concentrating on individual uniformities across five countries (U.S., U.K., the Netherlands, Austria and West Germany) rather than documenting national differences. The focus is on unconventional and direct political tactics—not necessarily violent—such as petitioning and demonstrating. These are viewed as a concomitant of post-materialism in the five countries, the argument being that not only must education and extended leisure increase participation, but also changing values and outlooks, particularly of the middle class, will alter the content of political participation. It will decreasingly take the form of broad socio-political movements for system change, and entail instead "an increase in limited, issue-bassed . . . ad hoc group actions" (p. 49). To an ever-greater extent, these will express themselves directly rather than through parties and pressure groups.

This is an interesting thesis, touched off by the student protests of the late sixties, but not necessarily less valid because of that. How does it stand in light of the analysis presented in this book? The authors would be the first to admit that their answers cannot be conclusive. Limited by time and resources to basing themselves on single cross-sectional surveys for each country, they can only extrapolate tendencies backwards and forwards in order to estimate changes over time. These limitations also prevented them from uncovering much evidence on overt protest behavior (a point which in itself rather detracts from the assertion that such behavior is widespread).

As a result, their dependent variable is not, as the book-title misleadingly implies, political action, but attitudes towards political action: in particular, whether particular forms of direct action are approved or disapproved, and whether respondents think they might ever undertake them in the future (having not done so in the past).

Approval of political action is viewed as affecting actual behavior not only by creating individual dispositions to undertake protest but also by producing a general climate of tolerance towards its emergence. The assertion is again plausible but untestable in the context of the book. Curiously, the country in which the "license for protest" is strongest is the Netherlands. In spite of the tendency for Amsterdam squatters to take to the streets, the Dutch are not noticeably more prone to protest than Americans, Germans or the British. Their replies here remain anomalies not resolved by discussion.

The analysis in general rests too heavily on the assumption that measuring attitudes toward protest is measuring protest behavior itself. Rather than concentrating on explaining such attitudes through other attitudes, the authors might have paid more attention to the question of just how far they do trigger action in the countries studied.

This said, many of the findings on attitudes are of interest. Approval of protest is certainly more evident among the educated young than among other groups—perhaps a pointer to the future? Moreover, approval of direct action is, if anything, stimulated by a heavy engagement in conventional political activity—there is no antithesis between the two. This finding certainly undermines any assumption that protesters are uniformly opposed to all aspects of the existing political system. For many people, direct action is an alternative to other forms of participation to which they may resort when pushed. The various types of action, from lawful demonstrations to occupations and property damage, are clearly arrayed by all respondents in order of increasing severity and risk of violence (to the extent that they form clear and well-validated Guttman scales). Engagement in protest action seems to be generally seen as an escalation of tactics in response to ever more intolerable political alternatives, and this holds without regard to country. Governments be warned!

The careful validation and use of cumulative scaling, as well as the analysis of ideologies, illustrates the methodological care and rigor of the analysis which forms a great strength of the book. It is worthwhile recommending to students as an example of field research in which the classic methods of index construction are applied as originally intended and operational problems are discussed with clarity and honesty.

The major thesis is that (protest) action has increased, is increasing and ought to increase more. What evidence is there for the first two points? Here we come to the problem that bedevils survey research: the lack of comparable data over an extended, or even in this case a limited, time period. Apart from comparing the reported inclinations of parents and children, and age-cohorts, all the authors can do is to point to the indubitable upsurge in protest in the late 1960s and argue for its continuation since. This reasoning implies, however, that protest activity has always been at the low level of the 1950s and 1960s (though, arguably, in Britain, the Suez issue and the Campaign for Nuclear Disarmament did create a comparable earlier level of participation). However, harking back to the demonstrations and street-fighting of the interwar period, it is by no means obvious that present direct action has reached a higher level. It could be argued that it has tailed off in the seventies compared with the previous decade.

Much of the argument for an increase in protest behavior rests on the equally problematic assumption that increasing segments of the population have shifted from a concern with order and economic security to a concern with equality and freedom of speech. Such "post-materialist" values, it is argued, will prompt an increasing recourse to direct action. Since equality and freedom are traditional concerns of the left, particularly of its middle-class supporters, the same point could have made more economically by citing the increasing strength of socialist and radical alternatives over the postwar period in the countries concerned—more than enough in itself to account for any change in political tactics.

These slight critical points by no means invalidate the argument that protest activity is an established feature of political participation to which activists will resort if they feel the conventional channels are blocked, and to which nonactivists can be roused if the stimulus is sufficiently strong. If direct action has indeed been employed in most mass democracies under a variety of guises since their establishment, there is an even stronger case to be made for its recognition as part of the general political repertory available for citizens as a means by which to influence their governments. This book has not established all its points. It has, however, forcibly drawn the attention of political scientists to a neglected but vital form of democratic political activity. The further comparative investigation promised by the authors should shed more light upon this, especially if they can now concentrate on actual protest behavior rather than attitudes which are only inconclusively related to it.

IAN BUDGE

University of Essex

Micropolitics in Contemporary China: A Technical Unit During and After the Cultural Revolution. By Marc J.. Blecher and Gordon White. (White Plains, N.Y.: M.E. Sharpe, 1980. Pp. xiii + 135. $17.50.)

This impressive little study is a product of the authors' exploitation of a unique interview opportunity in Hong Kong. In the modern China literature, it augments a shelf of very useful case studies of organizational microcosms—an urban neighborhood or a commune here, a ministry or a county administration there—that invite limited testing of broader interpretations. Marc Blecher and Gordon White actually improve upon the genre by entertaining a few leading hypotheses about the Cultural Revolution period themselves.

The result is more or less to confirm the main conclusions of Hong Yung Lee's *The Politics of China's Cultural Revolution* (Berkeley, 1978). A loose "rebel faction" allied in this unit in 1968 most of whose members and activists were people of middling or bad class background, old friends, non-party members, technicians (as opposed to workers or administrators) and particularly technicians in positions of responsibility. These "rebels" expressed through their style of political participation an acute sense of discrimination, and consequent frustration, under the party dictatorship of the mid-1960s. They acted in the desperate self-interested hope that the system might be changed.

By 1973, the last year of available data for this unit, their hopes had been dashed. The pre-Cultural Revolution party organization had essentially reconstituted itself, and everyone had grown "tired of politics." The later "Gang of Four" and "Four Modernizations" phases, unfortunately, could not be included.

Preceding their account of the Cultural Revolution, Blecher and White describe the unit in detail, including its personnel structure and operations, as well as the general political issues that had arisen there. These details are of more than usual interest because the case study shelf at present contains no other work about a technical body. The unit was located in an interior province and was responsible for some sort of field surveys. Almost half of its 240 employees were technicians. It was responsible ultimately to a ministry in Beijing (Peking).

Description and hypothesis testing both rest on a data base of 25 items of information about each and every employee of the unit, a treasure trove unheard of in the modern China field. The authors are most careful in presenting their data with appropriate cross-tab statistics.

The offbeat source of this wonderful archive might give some readers pause. It is a single informant, "Mr. Ji," a former employee of the unit. With "his encyclopedic memory" he was able to recall the data in 100 hours of interviews lasting over seven months. Blecher and White are sensitive to the obvious problems, and they make a persuasive case for Mr. Ji's remarkable mental powers. As a clincher they told him late on that they had lost their notes on factional membership and asked him to review each person's faction.

"To our amazement, he did so on the spot without a single mistake" (p. 7). I am persuaded.

GORDON BENNETT

University of Texas, Austin

World Leaders: Heads of Government in the Postwar Period. By Jean Blondel. (Beverly Hills, Calif.: Sage Publications, 1980. Pp. 282. $17.50, cloth; $7.95, paper.)

Jean Blondel's latest work is unfortunately neither thoughtfully conceived nor carefully executed. It aspires in a modest volume to analyze the structure, background, career pattern, and tenure of the top leaders of some 138 nations from 1945 to 1975. While this would seem the stuff from which cross-sectional number-crunching follows, Blondel's analysis, dotted with bivariate cross-tabulations, nowhere tests for strength of relationship or statistical significance. This abstinence might well portend, from a senior scholar, more informed and substantial insight as a welcome relief from purely statistical prowess.

But this is not to be. Blondel makes a number of implicit (i.e., undefended) assumptions about his data, all of which are objectionable. He assumes that each of his 138 nations are of equal weighting. Population size or economic strength are not considered as variables for analysis. So we get findings like this: "Only six countries outside these two areas [Latin American and sub-Saharan Africa] are ruled by a president: the United States, Algeria, Cyprus, the Philippines, Indonesia, and the Maldive Republic" (p. 57).

Blondel assumes that each nation is sovereign, with equal capacity to choose its own top leaders. The really interesting questions are where, when, and in what manner this assumption approaches credibility. Nowhere does the author take the opportunity to raise the question of outside intervention and its effects on leadership choice. One entire chapter, devoted to "routes to leadership," nowhere mentions the role of foreign intervention. In a world of sovereign, semi-sovereign, and non-sovereign systems, this lumping together is a remarkable failure in concept.

Consistent with the above, Blondel asumes that formal governments actually govern, that top titleholders are in fact the top leaders. "All countries have a government, and all governments have one or more persons at their head. Everywhere, leaders of government are viewed as essential elements of the political and social life of their country" (p. 37). For the most part, the study confuses officeholding with power. While recognizing the power of party leaders in the communist systems vis-à-vis government officeholders, Blondel will not deal with the de facto power of IMF officials in Zaire, or the U.S. Ambassador in Saigon (San Salvador), the Soviet Ambassador in Prague (Kabul), or a United Fruit executive in Guatemala. One theme developed by Blondel is that of dual leadership, especially in the communist systems, yet he is unable to extend this theme beyond his concept of the nation-state, to an area where it might break new ground.

The main text is filled with one and two-line analyses of entire nations over extended periods. Some seem accurate enough; some, such as the coronation of Jean Bokassa in the Central African Empire as an example of the potential for new monarchies, seem poorly grounded (p. 57); all are unsatisfying. As I have argued elsewhere, it would be preferable to select a smaller number of cases to provide for greater depth and a closer fit to key assumptions.

This study is intended as the first of a three-volume series, yet there is little effort to resolve relevant methodological issues. The author attaches considerable importance to making a dichotomous distinction between "regular" and "irregular" succession at the top (Ch. 3). Yet, aside from stipulating that "regular" transfers of top office meet the letter of existing law, Blondel tries to deal with the problem of a "climate of pressure" as follows: "All that can be said is that, where there is a broad consensus that the pressures are 'acceptable,' the resulting transfer is 'regular' " (p. 84). *How* broad a consensus, and acceptable *to whom,* are not defined; again, external pressures are not mentioned. As Blondel is aware, reliable information on "acceptability" is not generally available, and the author gives no indication as to how a determination of "regularity" was made for this study.

Some of Blondel's findings, on the identification of dual leadership in the communist systems, on the social background of top officeholders, and on routes to top title, have been better established and refined in works by John Hodgson, Robert Putnam, Ralph Miliband, and Klaus von Beyme, to name only a few. For whatever reasons, the author takes no notice of these works. Other findings, that long-term officeholders are more likely to enter office at an earlier age (p. 226), and are more likely to exit through death or retirement (p. 205) are not enlightening.

Finally, the volume suffers from annoying technical inadequacies. Several tables (pp. 90, 92 and 122) have percentages placed immediately next to frequencies with which they are *not* associated. Table 6 (p. 107) gives column totals for frequencies, but calculates percentages across rows. There is, on the other hand, no table of the nations included in the data set; the tiny bibliography is not useful.

JOHN D. NAGLE

Syracuse University

Planning and Budgeting in Poor Countries. By Naomi Caiden and Aaron Wildavsky. (New Brunswick, N.J.: Transaction Press, 1980. Pp. xviii + 371. $7.95, paper.)

In the six years since it first appeared in 1974, this book has become a minor classic among studies of public administration in developing countries. It does not deal with dramatic issues of ideology, or political conflict, or development strategy, or even the North-South confrontation. Instead, it focuses on the humdrum of Third-World public administration, the processes by which financial resources are allocated by governments, and specifically, the relationship between macroeconomic planning and annual budgeting. What accounts for the success of this book, and for the publisher's confidence that a paperback edition will sell in the 1980s?

The argument is straightforward: macro-economic, multi-sectoral, "comprehensive" planning has been a disastrous failure everywhere it has been tried. Because it requires accurate data, abundant economic resources, and the power of planners to impose their priorities and designs on regimes that are inherently pluralistic, macroeconomic planning is especially unsuited to countries characterized by poverty, uncertainty, and instability. Whenever planners attempt to step down from the world of abstract calculation and confront the realities of their societies, they become embroiled in the normal processes of decision making and their activities become indistinguishable from others attempting to cope with uncertainty, limited resources, competing goals, and conflicting demands. Instead of imposing predictability on society, the latter succeeds in absorbing the planners. The solution is to discard the wasteful, futile, and pretentious efforts at comprehensive planning and to rely instead for governmental resource allocation on the more modest process of budgeting, a hitherto neglected subject in the literature on development administration. Close to the shifting and often unpredictable realities of economic life and to the power centers of governments, the budget authorities, strengthened by modest, incremental improvements, provide the most promising avenue for strengthening management in the governments of poor countries.

This book appears to have been conceived in the mid-1960s at a time when macro-economic planning was the dominant fad among developmentalists and economic rationality was heralded as the social technology that would insure the achievement of modernization and the maintenance of the affluent society. The U.S. government was requiring that all recipients of U.S. foreign aid produce national multi-sectoral econometric plans; economics was becoming the master science for public policy; and PPBS was being installed in all the departments of the U.S. government. By the time this book appeared in 1974, the bubble had burst; PPBS had been abandoned in the federal agencies, and "new directions" had been instituted in U.S. foreign aid programs and in the World Bank. These new directions de-emphasized macro-economic growth, large-scale capital flows, and financial investment—the forte of the planners—and emphasized rural development, income distribution, popular participation —subjects that were unfamiliar to devotees of "takeoff" and "the big push" and for which economist-planners enjoyed no comparative intellectual advantage. The intellectual debunking of comprehensive national planning had been well under way before this book helped to administer the coup de grace, and not without a substantial element of overkill. The authors are inclined to accept at face value the testimony of planners about their limitations, failures and frustrations, while dismissing the benefits of longer time horizons, economic rationality, inter-sectoral consistency and similar useful perspectives which planners have brought to development policy in many governments.

To students of development administration, the uniquely useful and fascinating contribution of this book is its behavioral treatment of public budgeting. Budgeting is a subject with a rich history of normative and prescriptive writing oriented to tighter control and increased efficiency in the use of public funds. Comprehensive budgeting, program budgeting, performance budgeting are among the normative canons that have filled the literature on financial administration and provided the rationale for a significant flow of international technical assistance. But though every observer and practitioner has long known the practical tricks of the trade, which depart markedly from the prescriptive literature, they have been of little interest to scholars. Few of these practices escape the pages of this book—from the calculated underestimation of revenues, arbitrary ceilings, incremental authorizations, political bargaining, and releases based on available cash flows on the part of central budget authorities, to the padding of estimates, concealing of resources, special earmarking of funds, and deliberate underestimation of multi-year capital requirements by operating departments. From the perspectives of the parties concerned, the authors demonstrate that these behaviors are rational and are exacerbated by the uncertainties and the desperate shortages of funds that chronically confront the governments of poor countries:

> The pervasive influence of poverty permeates government budgeting in poor countries. Faced with multiple uncertainties they resort to tactics which give them room to maneuver. They favor short-term over long-term commitments, a generalized conservatism over exactly programmed outlays, political bargaining over economic analysis, and figures which can later be moved over hard-and-fast allocations. They desperately seek redundancy. The elaborate and often exasperating games which result are thoroughly grounded in the needs of spenders and guardians to create and defend a surplus when both are poor. The effect, as with so many other facts of life in low-income countries, appears to be perpetuation in a vicious cycle of misery, muddle, and mendacity. The conclusion drawn is that, as it is now organized, government financial administration is powerless to help the economies of the poor nations (p. 165).

As with so much behavioral writing, this book is far more convincing in description, analysis, and criticism than in prescription. Caiden and Wildavsky argue that macro-economic planning should be abandoned; emphasis should shift to improved budgeting, to "converting obstacles to opportunities," as they bravely put it. But when they attempt to be concrete, their proposals prove to be either very modest or impracticable. Improving staffing, cost-accounting, and financial reporting systems to increase the learning and error-correcting capabilities of governments are unexceptionable proposals dating back at least to Point IV in 1949. The proposal to reduce uncertainty about levels of revenue by converting foreign aid from project assistance to general budget support appears to be innocent of the extensive literature on the subject, including the failure of program assistance to influence the economic and fiscal behavior and the priorities of recipient governments, and the inability of donor agencies for their own political reasons to allot funds in that way. "Continuous budgeting" to facilitate adaptation to "emergent problems" recognizes that budgets are continuously being modified in response to contingencies both on the revenue and the expenditure side; it is therefore presented as a substitute for the formalism and the rigidities of annual budgeting. Nowhere is there an estimate of the institutional implications of this proposal—a process of financial allocations and presumably tax assessments dominated by finance and departmental bureaucrats from which political authorities would be excluded even more than at present. The concrete proposals for reform are disappointingly meager and unpersuasive and, not surprisingly, have had no influence on policy, despite the wide readership this book has gained.

In the continuing tension in the social sciences between the "rationalists" and the "incrementalists," this book comes down powerfully on the side of muddling through. But muddling through is not, according to Caiden and Wildavsky, the absence of rationality, but rather rejection of the abstract rationality of planners in favor of the pragmatic rationality of the numerous actors in government pursuing plural goals in an environment dominated by uncertainty and chronic resource scarcity. By imputing a higher rationality to the current actors, by celebrating their pragmatic coping and problem-solving behavior, and by their evident hostility to social engineering, Caiden and Wildavsky come close to the conclusion that, given the circumstances, whatever is, is rational and right. This attitude is compatible with the present mood in the social sciences which is skeptical about government intervention and reform, and with the powerful drift to neo-conservatism in public affairs. The success of this book has been due, in my opinion, to its devastating attack on the arrogant and over-intellectualized rationality of comprehensive economic planning as a form of large-scale social manipulation; to its refreshing description and explanation of budgetary behavior in the turbulent and resource-constrained conditions of poor countries; and to its skepticism and about a positive role for government in reforming existing institutions which enhance economic efficiency or improving living conditions in developing countries. The message of this attack on planning is reliance on the operation of pluralistic political and bureaucratic markets to formulate public policy and manage public affairs; there is no concession to the insights of the planners that the desperate poverty of these countries might call for a more dirigist use of public authority, even though the risks might be high. There is little in this book that suggests how public affairs in poor countries might be better managed and even less about substantive priorities and the organizational means of achieving them.

MILTON J. ESMAN

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The Social Democratic Image of Society. By Francis G. Castles. (Boston: Routledge & Kegan Paul, 1978. Pp. xiv + 162. $15.50.)

Francis Castles has provided a perceptive analysis of Scandinavian Social Democracy in this short comparative survey of party origins and policy achievements in Sweden, Norway, and Denmark. Country specialists and scholars who use different measures of social welfare will no doubt fret about various omissions and interpretations of empirical data; but overall Castles' achievement is an impressive *tour d'horizon* of an important set of case studies of long-term Social Democratic efforts to humanize industrial-capitalist society.

The stated purpose of the volume is "to explain the political dominance of the Scandinavian Social Democratic parties and to demonstrate the consequences of that dominance" (p. x). To that end Castles describes, in his first chapter, contrasting patterns of Social Democratic and union development in the three Scandinavian countries, the parties' strategies for maintaining their status as the dominant political force in Scandinavia, and the emergence of "new problems and new parties" during the turbulent 1960s and 1970s. In his second chapter Castles addresses himself to the most complex and controversial theme of his study: namely, whether or not Social Democracy in Scandinavia "has made a fundamental improvement in the conditions of the working class. . ." (p. 50), and, if so, according to what criteria, and for what reasons. The concluding chapter (part 2 of the book, paradoxically labeled "Origins"), surveys sources of Social Democratic electoral support, class-group relations, and the weakness of the Scandinavian right.

Castles' study is not based on field research of his own, nor does it purport to present any substantive new findings. Instead, Castles' strength lies in his discerning summary and critique of contrasting perspectives on the Scandinavian experience. These range from what Castles describes as the "reformist interpretation," which largely celebrates Social Democratic policy achievements since the early 1930s and holds that they are replicable elsewhere, to the "radical interpretation," which "denies the Social Democrats any genuine achievement and presumably regards their continued historical success as a basically unimportant historical anomaly" (p. xii). Castles himself insists that "both interpretations are fundamentally incorrect," suggesting instead that the Scandinavian experience is singular and that generalizations from it are therefore "inappropriate."

To buttress this argument, Castles devotes most of the first and third chapters to a descriptive analysis of distinctive features of the Scandinavian Social Democratic movements. These include historical attitudes of tolerance on the part of traditional bureaucratic-conservative elites, which facilitated the peaceful emergence of democratic socialist unions and parties during the latter part of the nineteenth century; the close institutional and programmatic links between the parties and the national federations of labor in contemporary Scandinavia; and above all the Social Democrats' striking record of policy success (particularly in Sweden) in extending social services and facilitating a largely sustained pattern of economic growth during the past four decades. Underlying the latter achievement, Castles notes, is the fact that the three Social Democratic parties have consistently retained their character as working-class parties. By retaining the ideological trust of most industrial workers, they have thus maintained their status as the dominant party on the left throughout Scandinavia—thereby escaping the paralyzing division of the socialist movement in various other European countries while simultaneously achieving the integration of the working class within the larger social fabric on the basis of institutionalized power. It is this feature of Scandinavian society and politics, Castles contends, that accounts for the maintenance of relative harmony on their labor markets—and not, as other observers have claimed, the prior existence of consensual norms and institutionalized procedures for resolving conflict.

An important further indicator of the "insularity" of Scandinavian Social Democracy is the effect that Social Democratic policies have had on "conditions of the working class." In the lengthiest and most problematic section of his book, Castles addresses himself to this fundamental issue by seeking to evaluate empirically the consequences of welfare reform measures in an explicit comparative framework. He begins with a brief summary of prevailing radical criticisms of reformist Social Democratic parties (notably Frank Parkin's *Class Inequality and Political Order*, Praeger, 1971), and readily concedes that the Scandinavian Social Democrats have in fact failed to modify the basic "reward structure" of capitalism or to achieve social equality. But they have made a meaningful difference in the way people live, as demonstrated by their capacity to effect welfare state reforms.

Castles utilizes a composite "measure of welfare state provisions" to assess the social and economic performance of the 25 countries that comprise the Organization for Economic Cooperation and Development (OECD). His measure consists of four social indicators: (1) the current revenue of the general government as a percentage of the Gross Domestic Product (GDP); (2) total public spending on education as a percentage of the Gross National Product (GNP); (3) infant mortality; and (4) per capita GDP. Various problems are immediately apparent in this choice of indicators. Castles himself notes that they offer only an "approximation of the reality of welfare state provisions" (p. 66). Moreover, their choice lacks any compelling theoretical justification and is patently subjective (though by no means arbitrary). Nonetheless, Castles' effort to devise an applicable empirical measure of welfare state performance is a noteworthy improvement over the more superficial assessments of the Scandinavian experience advanced by both the reformist and radical interpretations.

In applying his index of welfare provisions, Castles finds that Sweden and Norway rank first and second, respectively, and Denmark ranks fourth in a rank ordering of the OECD nations. (The Netherlands ranks third, Canada ranks fifth, and the United States tenth.) Interpreting these and related descriptive statistical data, Castles concludes that "the Scandinavian Social Democratic claim that 'we're far ahead compared to other countries' is justified. With the single exception of the Netherlands, no other advanced countries manifest anything like the same consistently high levels of welfare effort and output" (p. 73). This achievement, in Castles' view, is demonstrably a product of political choice—specifically, the Social Democratic commitment to cultivate welfare—and not a generalized consequence of levels of economic development.

As is the case with any macro assessment of complex social phenomena, Castles' study can be faulted on various grounds. For example, he does not fully capture the complexity of the Danish party system nor adequately probe the recent volatility of the Scandinavian electorate. Nor does he utilize relevant documentary material to explain sufficiently the potentially explosive issue of wage-earner funds—a concept advanced since the mid-1970s by union leaders in Denmark and Sweden as a means to extend labor's influence vis-à-vis private capital. To have done so would have enhanced the book's indisputable status as a thoughtful and provocative assessment of the distinctiveness of Scandinavian Social Democracy. Whether the resulting debate on the prospects of economic democracy, as a step beyond the welfare state, will mean that the Scandinavian experience can remain insular is, of course, another question.

M. DONALD HANCOCK

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Supermadre: Women in Politics in Latin America. By Elsa M. Chaney. (Austin: University of Texas Press, 1980. Pp. 210. $14.95.)

This work is the first book-length study by a single author published in the United States about women in Latin American government. The core of the study is an analysis of women leaders in Peru and Chile, based primarily on 167 interviews with female politicians and officials in 1967. Elsa Chaney sets this analysis in a broader context by briefly reviewing the image of women in Latin American culture, the history of the women's movement in Peru and Chile, and more recent trends in women's participation in public life in the two countries. The different facets of the study are bound together by a single theme: women in Latin America, to a greater extent than elsewhere, are restricted in their professional lives to stereotyped maternal roles.

As one of the very few studies of women in Latin American politics, this book is a pioneering work. The selection of Peru and Chile was an informed decision, allowing Chaney to make generalizations to Latin America as a whole because Peru was considered one of the most "traditional" South American nations and Chile one of the most "modern." (One intriguing finding of the study is that, contrary to the conventional images, the attitudes of the Peruvian and Chilean women leaders were similar.) The many revealing quotations from women leaders indicate that Chaney was a skilled interviewer. The combination of the rich interview material with background overviews of women in Latin American culture and history in one brief book will make the work particularly useful for many readers.

Yet, in part because of the pioneering nature of the study, it is flawed both methodologically and conceptually. Chaney cannot present a methodologically rigorous case for her main argument because the necessary comparative and time-series data are not readily available. To show that women in Latin America are more strictly bound by maternal stereotypes than women elsewhere requires comparable data for women outside Latin America, but such data are absent. For some control group data, interviews with Peruvian and Chilean male leaders might have been conducted; comparison between the men and women would have been particularly interesting with respect to the leaders' priorities for national development and their views of their own careers. Chaney's argument would also have been strengthened by the inclusion of more current data. Completed in 1967, Chaney's interviews predate the emergence of the women's movement even in the United States. Chaney suggests, but does not demonstrate, that Latin American women's attitudes have remained stable over 13 years.

Although these methodological deficiencies were probably beyond Chaney's control, some methodological improvements were perhaps feasible. We need more information on the representativeness of the 167 interviewees, especially for the Peruvian sample. Chaney also tells us little about her selection of 12 women in each country for extended informal conversations and observation of them at work. One indication of Chaney's inattention to methodological detail is that she reports on page 107 that she observed "most" of the selected women officials at work but on page

131 that she observed "each one."

Even more important to Chaney's argument is a careful conceptualization of the maternal stereotype and explicit specification of the attitudes and tasks within this classification. Chaney acknowledges the significance of this issue in the introduction and conclusion of the book. She points out that conceptualization is difficult, especially because some analysts, in particular Latin American ones, may perceive the designation of certain abilities and tasks as uniquely "feminine" to be positive rather than negative. Yet, in the body of the text, Chaney does not acknowledge this analytical issue. Nor does she advance a working classification of attitudes and tasks under the "feminine" rubric. At times Chaney seems to stretch the definition of the feminine role so that the attitudes and activities of the women leaders fit into it. For example, in Chaney's analysis of interviewees' statements as to what, upon their hypothetical election to the presidency, their administration's three main goals would be, Chaney classifies the responses "more jobs" and "more, cheaper foods" as " 'housewifely' adjustments" (p. 146). This designation might interest Peru's recently elected president, Fernando Belaúnde Terry, who made the promise of a million new jobs a cornerstone of his campaign.

Despite these flaws, this book is a useful starting point for the analysis of women in Latin American politics. Its data will provide a helpful baseline for studies in the 1980s. Its argument is provocative and will influence future work.

CYNTHIA MCCLINTOCK

George Washington University

The Soviet Union Since Stalin. Edited by Stephen F. Cohen, Alexander Rabinowitch, and Robert Sharlet. (Bloomington: Indiana University Press, 1980. Pp. viii + 342. $22.50, cloth; $7.50, paper.)

Readers who have grown dubious about the usefulness of multi-authored studies may be pleasantly surprised in reading this volume. The general theme of developments in the Soviet Union since Stalin is focused enough to give the book internal coherence and yet open-ended enough to provide latitude for the individual authors. Specialists on the various topics covered in the book are unlikely to find much that is new here—indeed, several of the contributions have been previously published. But taken as a whole, the volume provides a broad analysis that transcends narrow specializations and will therefore be useful and informative to most persons interested in the Soviet Union.

Political scientists are likely to find the set of essays on "Society and Culture" the most enlightening, particularly George Gibian's sensitive analysis of "New Aspects of Soviet Russian Literature." Gibian not only provides a concise review of recent Russian literature, but he also analyzes the political dimensions of Soviet culture, which has no close analogue in the West.

The volume is divided into four parts: Politics, The Economy, Society and Culture, and Foreign Policy. The essay likely to have the most long-term importance opens the book. This is Stephen F. Cohen's "The Friends and Foes of Change: Reformism and Conservatism in the Soviet Union." Previously published as the lead article in the June, 1979 *Slavic Review* (and published here without the four short commentaries on the piece by other specialists and Cohen's reply to them), Cohen's essay attempts to provide a fresh look at the major forces that have determined the direction of Soviet policy in recent years. The most important mainstream attitudes he labels "reformism and conservatism," and he concludes that the Soviet Union under Brezhnev has become "one of the most conservative countries in the world." This short description does not capture the subtlety and thoroughness of Cohen's interesting analysis. Particularly noteworthy is Cohen's point concerning the problem ideology poses for the dominant conservative position in the present leadership: that even in dogmatized form, Marxist-Leninist ideology is based on the idea of change, and thus it serves as a useful tool for the reformist persuasion in the present leadership.

Probably second in importance is the last substantive essay in the book, that by Charles Gati on "The Stalinist Legacy in Soviet Foreign Policy." Gati argues, in effect, that the basic nature and approach of Stalinist foreign policy is still used by his successors. This is no mere piece of cold-war rhetoric, but a well-reasoned, thoroughly argued statement (which fits in well with the theme enunciated by Cohen concerning the essentially conservative nature of the present leadership). It is also a controversial position to take, and the overall value of the book is enhanced by the inclusion of three short "responses" to Gati's essay, by Roger E. Kanet and John D. Robertson ("The Soviet Union and Eastern Europe"), Alvin Z. Rubinstein ("The Soviet Union and the Third World"), and William Zimmerman ("The Soviet Union and the West"). Zimmerman's statement, which emphasizes the distinctions between Stalin's foreign policy and that of his successors (and which prompted Gati to write an afterword to further dispute Zimmerman's position), does a particularly good job of sketching the contours of the controversy.

Between the Cohen and Gati essays are 11 other pieces. "The Stalin Question," by Soviet dissident Roy A. Medvedev (adapted from Medvedev's 1979 book *On Stalin and Stalinism* and the only piece not connected with the conference at which these papers were originally presented) provides interesting details by a semi-insider on de-Stalinization and the attempted rehabilitation of Stalin in 1969. George Breslauer's "Khrushchev Reconsidered" provides a useful supplement to Cohen. Breslauer shows how the Brezhnev-Kosygin regime has been a "conservative reaction" to a number of Khrushchev initiatives, which has resulted in, among other things, "the abandonment of populist efforts to redefine the status of political authority." His conclusions should give pause to those who argue that popular participation has been significantly enhanced under Brezhnev. Essays by Erik P. Hoffmann and Robert Sharlet complete the section on politics and provide different perspectives on policy making.

Three essays in the second section of the book give an overview of current problems in the Soviet economy. Probably of most interest to readers will be Robert N. Taafe's "Soviet Regional Development." Among other things, the author gives considerable attention to problems growing out of the fact that much of Soviet energy resources are found in the eastern part of the country, while energy needs are overwhelmingly located in the west.

In addition to the aforementioned chapter on literature by Gibian, there are three other essays in the section on society and culture: John Bushnell's analysis of a variety of public opinion sources, which leads him to conclude that the Soviet "middle class" has become increasingly pessimistic about the prospects for continued material advances; Ronald Grigor Suny's interesting case study of Georgia and Soviet nationality policy; and Peter H. Juviler's chapter on the Soviet family, which includes fascinating information on women "in rebellion."

As with all collections of this kind, the essays vary somewhat in quality. In general, however, the level achieved here is quite high. The book can be warmly recommended as a useful source on the Soviet Union in the seventies.

DONALD D. BARRY

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Development and Diffusion of the Nuclear Power Reactor: A Comparative Analysis. By Peter deLeon. (Cambridge, Mass.: Ballinger, 1979. Pp. xvii + 324. $17.50.)

Peter deLeon examines the research, development, demonstration and diffusion of "slow" nuclear fission power reactors in the civilian sectors in six countries—the United States, Canada, Britain, France, West Germany and the Soviet Union. He is interested in the relative importance of four objectives—science for science's sake, national prestige, development of efficient technology, and political equity. Furthermore, he relates seven attributes of civilian technology development to these objectives. Among the attributes he examines are the existence of a concomitant military program, the existence of a technology delivery system in each country and the role of an active scientific community. The actors he examines are the organizations and bureaucracies within each state, rather than, at one extreme, the nation state writ large or, at the other, the role of individuals. These organizations, e.g., government bureaus, the nuclear construction industry and the electric utilities, play different roles at the various stages of the technology development process.

The book contains a wealth of detailed information on the comparative politics of nuclear energy in highly industrialized states. It is clearly organized and written, but the attention to detail could very well overburden the reader who is not a specialist in nuclear power. However, there is a handy glossary to assist the reader who may be overcome by too many acronyms.

As a result of the relationships derived from case studies which are compared according to the framework described above, deLeon offers "the basis for a testable set of policy hypotheses that point in the direction of a general model of government-sponsored civilian technology developments" p. 274). But, as he himself says, his model can only offer modest predictive claims. However, he does view it as a policy analysis tool which can be useful in explaining the innovation of other civilian technologies, but he does not wish to claim too much or become procrustean. He quotes Abraham Kaplan approvingly, "We tinker with the model when we might be better occupied with the subject-matter itself" (p. 254).

Furthermore, deLeon mentions possibilities for new objectives and attributes which he did not include in this study, e.g., "national culture" and "technology for export" (p. 276). This line of thinking must lead to the conclusion that what we have is a heuristic framework rather than a model in a scientific sense.

There is one conclusion that deLeon might have emphasized had he written since the Three Mile Island accident and the economic realities of today, i.e., "The early commercialization of advanced technology might be a mixed blessing" (p. 250). This points to the fact that he leaves out a possible stage of the technology development process—the death or the decline of the efficacy of the technology on grounds of lack of prestige, efficiency and equity. Another lacuna in the analysis is that, while deLeon compares nuclear power reactors to fossil fuel plants, a comparison to renewable and decentralized sources of energy is lacking. A third area of analysis that could have been undertaken would be to stress transnational and geocentric approaches to technology diffusion by emphasizing the role of transnational firms and regional actors.

These are some areas for further thought and examination; they do not reflect on the excellent contribution deLeon gives us on the comparative politics of technology development and commercialization. It is a sound and thoughtful addition to the literature on technological change.

JONATHAN F. GALLOWAY

Lake Forest College

Democracy and Development in Turkey. By C. H. Dodd. (North Humberside, Great Britain: Eothen Press, 1980. Pp. 226. $22.50, cloth; $12.00, paper.)

This timely book appears when an increasing number of observers wonder whether Turkey's democratic political system will survive. Reports of violent conflict between various extremist groups, ethnic and religious conflict, and assassination of prominent political figures appear almost daily in the Turkish press. As its title suggests, this book offers some insights into the question of the development and survival of democratic politics in Turkey.

Democracy and Development in Turkey is perhaps the first general work on Turkish politics written from the perspective of a social scientist. Orientalists and historians have authored most general works on Turkish society. C. H. Dodd brings a fresh interdisciplinary perspective to his work and provides an informative and stimulating synthesis of much of the best and most recent scholarship on Turkish society. *Democracy and Development in Turkey* is also well written and well organized. After an introduction to Ottoman and Turkish history and society, Dodd examines political elites, political culture and political ideas, the normative framework of the Turkish Constitution, political parties and voting, the military and the bureaucracy, unions and industry, and students. He concludes with a valuable discussion of the prospects for Turkish democracy.

In most instances, Dodd is sensitive to different views among scholars. One example is his treatment of the question of whether the state or the economy was responsible for Ottoman decline. In a fair and balanced presentation, he persuasively suggests that the significance of economic factors was not as great as some advocates of dependency theory claim.

One of this book's significant contributions is its analysis of Turkish political culture and ideas. Dodd describes the political implications of such cultural themes as trickery and intrigue, strains of egalitarianism coexisting with a perception of the state as *devlet baba* (father state). He then goes on to examine these and other cultural themes in the light of available empirical studies.

A work which covers so many important areas is likely to raise some questions about interpretation. Dodd is perhaps overstating the case when in discussing the Kemalist period he speaks of "the notorious secularist regime of Mustafa Kemal" (p. 5) and "the massive secularization of the Atatürk revolution" (p. 76). Such remarks ignore the complex realities of the Kemalist era in which both Islam and local authorities were more important than many have hitherto acknowledged. An excellent treatment of Atatürk as "Champion of Change or Camouflaged Conservative" can be found in J. A. Bill and C. Leiden, *Politics in the Middle East* (Little, Brown, 1977), pp. 191-96. Similarly, Dodd tends to discuss Atatürkism as an agreed-upon set of principles. In fact, it is a hotly debated issue and is probably closer to a symbol which conflicting groups attempt to use for their own purposes. Dodd's excellent discussion of the military should have noted the growing concern expressed in the Turkish press about its increased politicization (and of many police units as well).

These reservations notwithstanding, Dodd's book is one of the finest introductions to Turkish politics and society available today.

ARNOLD LEDER

Southwest Texas State University

Government Versus Trade Unionism in British Politics Since 1968. By Gerald A. Dorfman. (Stanford, Calif.: Hoover Institution Press, 1979. Pp. vii + 179. $12.95.)

In this book Gerald Dorfman traces the attempts of successive British governments during the decade 1968-78 to curb the political power of trade unions, particularly of the Trades Union

Congress (TUC). Following an introductory chapter, which treats organized labor's postwar emergence as a force capable of paralyzing national economic policy and which summarizes the book's central argument, the next three chapters chronicle the governments' "counterattacks." A final chapter examines the impact on union-government relations of Britain's membership in the European Economic Community.

The main strength of the book lies in the brief histories of TUC interaction with the governments during the period. These accounts are grounded upon a large number of interviews (more than 40 are listed in an appendix, and other anonymous interviews are mentioned) with nearly all of the main participants. Dorfman weaves his materials together skillfully to create a richly textured narrative. I especially admire his ability to write concisely without being overly parsimonious.

Dorfman's analytical perspective is unusual, and—despite the clear language in chapter 1—I originally misread his purpose. Following the conventional assumption that the key issue in British labor-government relations was "Who governs?" I finished the book with a feeling of disappointment, for all the interesting description provided few new insights on this question. The power of the unions to bring governments to heel during the period studied was amply demonstrated and the governments' counterattacks generally seemed feeble in comparison; but this situation was already well known.

Only after I reread the first chapter did Dorfman's intent sink in. Parenthetically, the book lacks a conclusion; thus, a final opportunity to disabuse me of my confusion was lost. Dorfman sets out his argument in the book's initial paragraph: "In spite of periodic industrial conflicts, government for the first time is gradually winning the cooperation of trade unionism for effective economic management." Rather than focusing on the overall power quotients of labor and government as I had assumed, Dorfman focuses on governments' attempts to *tame* labor and draw it into a pattern of collectivist politics that "might work at last to produce economic stability or even prosperity" (p. 5). The prospect of success depends, he suggests, "on the development of coinciding incentives for unions and government to cooperate; or, at the least, on the development of government leverage which would effect union cooperation or perhaps acquiescence." Although the title and many passages in the substantive chapters reinforced my original assumption, the thread of Dorfman's argument can be traced throughout the book.

Even when measuring governments' victories only by their abilities to co-opt labor, however, the record Dorfman sketches is at best mixed. Harold Wilson sought to restore collectivist cooperation in 1968 by helping the TUC leaders regain authority from the shop stewards, but the warring factions perversely united against the government. Edward Heath's conversion to collectivist politics in 1972—following the twin debacles of the Industrial Relations Act and the miners' strike—was too late, and the TUC leaders were too divided to cooperate with the Tory "enemies." Harold Wilson (and later James Callaghan) gained the TUC's cooperation in implementing the "social contract," a relationship that was until mid-1975 mainly to the unions' advantage; then the continued cooperation, spawned by the deepening recession, was mainly to the government's advantage. Also in 1975, the TUC voluntarily became involved with the government in the affairs of the European Economic Community, both for defensive and offensive reasons.

With regard to this mixed record, Dorfman is optimistic but worried. On the left, his chief worry is that "union-Labor Party cooperation is still an unproven possibility in less than crisis situations" (p. 132). On the right, his chief worry is that the TUC will again find it impossible to cooperate with the Conservatives for ideological reasons and that "the next Tory Government will probably go out of its way to avoid entangling relations with the union movement" (p. 104).

The experience of the first year of Margaret Thatcher's government makes the latter statement seem prophetic. The *Economist* (17 May 1980, p. 47) has characterized the present situation: "In just 12 months the government has overturned what had become almost a constitutional convention—the right of the unions to be consulted on economic policy." Following monetarist policies and the principle of laissez-faire in labor negotiations, the Conservatives are quite interested in "Who Governs Britain?" And they are not interested in sharing power with the TUC. As Dorfman has pointed out, organized labor's power is limited by its negative aspect (p. 6). Because of the state of the economy, the government's intransigence, and the TUC's disarray, the unions have not been successful in focusing their power against the Conservatives; the spectacular failure of the TUC's "Day of Action" on 14 May 1980 is a case in point. Nevertheless, on the basis of Dorfman's able analysis of the previous decade of labor-government relations, one should not be surprised if such factors as Britain's climbing inflation and unemployment rates should produce political consequences compelling the Conservatives to reassess their position and seek the TUC's cooperation. The *Economist* article quoted above concludes: "How long will the new dispensation last?"

The book under review is a sequel to the author's *Wage Politics in Britain 1945—1967* (Iowa, 1973), and the new book might have been entitled *Son of Wage Politics*. Perhaps in due course Mrs. Thatcher will deserve a Dorfman book all her own, *Grandson of Wage Politics*?

LARRY B. HILL

University of Oklahoma

Political Manipulation and Administrative Power: A Comparative Study. By Eva Etzioni-Halevy. (Boston: Routledge & Kegan Paul, 1979. Pp. x + 218. $22.50.)

Eva Etzioni-Halevy intends this book as a study of "manipulation of the rank and file public" by political elites in Great Britain, the United States, Australia and Israel. Curiously, in view of the histories and analyses under which the library stacks groan, the subject is said to be "frequently alluded to" but "much less frequently analysed and documented" (p. 1). In point of fact, little new documentation is adduced in this book, which relies heavily for data on standard histories, secondary sources and even textbooks. While this innocent reader found the discussion of Israel informative, discussions involving the other three countries lack the nuance and sureness of feel found in works by writers in full command of their material. The mainsprings of American political history are particularly unappreciated; party competition, the vast range of party types across a continental society, the distribution of power and norms through time, the correlates of voting choices, the dynamics of reform movements, are all neglected or poorly understood as excessive (and poorly digested) attention is paid to the urban political machine as the presumed key to elite manipulation.

The book is rather over-titled as well: symbolic display is excluded from analysis on the grounds that "non-symbolic" manipulation, that is, the offer by politicians of material inducements to attract votes "poses a greater thrust to democracy than the former." Vociferous demagogism is said to be less of a "menace" because it is easier to "neutralize" by the media and oppositions (pp. 8-9). No doubt many scholars would reverse this emphasis. In this study, political manipulation is synonymous with "micro-corruption," e.g., payment to individuals of jobs, money, services, etc. in return for votes. It is said to have been virtually eliminated in the U.K. and Australia since its nineteenth-century heyday, to have declined greatly in the U.S., but to remain significant in Israel.

Declines in "political manipulation" (read "electoral corruption") have occurred mainly, it is argued, because public administration in the U.K. and Australia became separated from electoral politics, thus reducing the payoffs available, a "modernization" process that was partially "arrested" in the U.S. and Israel. Accompanying this, norms emerged which "restrained" elites from using public-sector resources as payoffs to individual voters. In the United States and especially in Israel, these norms are stil marked by a certain ambiguity. This may very well be correct, but the data introduced in evidence are often handled superficially, even wrongly. Consider one of Etzioni-Halevy's illustrations. The Nixon White House sought to use the Internal Revenue Service, she claims, to "embarrass political enemies. And if the attempt was not as successful as expected, this was allegedly not because Nixon's staff of public servants refused to cooperate or because the Internal Revenue Service resisted political pressures and regarded them as unethical, but because it had a greater commitment to the rival party" (p. 187). This is quite wrong. The canard that the IRS was uncooperative because it was "controlled by Democrats" originated with John Dean in his days of "blind ambition" in an undated memorandum to H. R. Haldeman. In it, Dean complained that IRS Commissioner Randolph Thrower (a Republican) had been unresponsive because he had become a "total captive" of the Democratic assistant commissioners. The next commissioner, Johnnie Walters (a Republican) was no more forthcoming. He had not shown "leadership," being either "reluctant or unwilling to do so." As a result, Dean wrote, "We have been unable" to "crack down" on "left-wing" foundations, to stimulate audits of or get information concerning "our political enemies," or to place "R.N.'s" people in the agency. On September 11, 1972 Dean met with Walters and turned over a list of 490 "enemies" to be investigated. Walters discussed the matter with Treasury Secretary Schultz (a Republican), who told Walters to "do nothing." Dean again met with Walters on September 25 and inquired into Walters' "progress" on the matter. Walters advised Dean that the matter should not be pursued, but agreed to bring it up again with Shultz. According to the Senate Watergate Report, "The matter was never taken up again, and the list which was given to Commissioner Walters was sealed and locked in his safe in the Commissioner's office." There were some instances of apparent abuse from lower levels in the IRS, but the facts are in dispute as to who was involved. In any case, Etzioni-Halevy's illustration is inaccurate. Thrower, Walters and Shultz (all Republicans) did in fact "refuse to cooperate," and did "resist political pressure" because they believed it inappropriate to use the IRS for political purposes, not because of an absurd "greater commitment to the rival party." In writing about personal corruption, there is a profound responsibility to get one's facts right. In other instances as well (which space limitations prevent detailing), although none as damaging to concrete individuals as this one, the lack of solid research is evident.

L. L. WADE

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India's Political Economy, 1947-1977: The Gradual Revolution. By Francine R. Frankel. (Princeton University Press, 1979. Pp. xiv + 600. $35.00, cloth; $12.50, paper.)

India's political economy presents a "basic dilemma: How can a gradual revolution be accomplished" that can reach its poorest people "without exacting fearful costs in human life. . ." (p. xiii). In stating the problem concisely and dramatically, Francine Frankel charts her middle ground between the two most commonly argued approaches to Indian development. In proposing what she terms a gradual revolution, she rejects alike a simplistic revolutionary approach and a simplistic accommodationist approach to India's basic problems.

Revolutionary theories are rejected on two grounds. First, they are not appropriate to Indian conditions which, Frankel maintains, center on religion, caste, and family. A national movement based on class appeals, therefore, becomes very difficult to organize. Moreover, a revolutionary solution could result in "incalculable levels of destructive violence" (p. xii).

An accommodationist approach, such as attempted by Jawaharlal Nehru and India's first generation of post-independence leadership, also is not seen as appropriate to Indian conditions. From an economic perspective, Frankel agrees with Gunnar Myrdal that evolutionary growth models will not resolve India's complex problems. Carefully conceived investments reach a dead end rather than serving as catalysts for self-generating economic development. Political accommodation in India rests on the key linkage role of dominant landowning castes which block radical social change, i.e., "a significant redistribution of income, status, and power to alter the structure of society" (pp. 23-24, 575).

Most of this substantial study is devoted to an impressive and careful elaboration of these central points. In the first four chapters, Mahatma Gandhi's and Nehru's approaches are described and analyzed. Both attempted to resolve what Frankel terms the "resolution of the paradox of accommodative politics and radical social change" (p. 24) by essentially avoiding direct confrontations with the dominant landed castes. On the other hand, rural elites would be gradually weakened by increasing political and economic mobilization of the poor. Gandhi emphasized a Constructive Program for village uplift which not only would improve social and economic conditions, but, the author maintains, was also an organizational device intended to weaken the dependence of the poor on the dominant landed castes.

Nehru's ambivalence during the pre-independence period is explored, particularly as he led class-based actions in Uttar Pradesh and supported the emergence of the Socialist Group in the Congress party. After independence, the Nehru-led leadership emphasized the importance of the Congress party as a national unifying force within an accommodationist political framework. At the same time, it was anticipated that a socialist approach to society coupled with new village-level institutions could lead to pressure from below so as to accomplish radical reforms.

Nehru's attempts to speed up the process as in the Nagpur Congress Session in 1959 failed as the landed elements interpreted the measures for agrarian change as resulting in "their own destruction" (pp. 162f.). In turn, new village institutions such as *Panchayati Raj* were captured by large landlords.

India's experience, Frankel concludes, shows that "attempts at social transformation through institutional change that stopped short of direct peasant organization only compounded the problem of democratic reform" (p. 200). Propertied groups, rather than succumbing to pressure from popular majorities, were adept at manipulating the fragmented peasantry. Political unity and stability accruing from the accommodationist approach were "purchased at the price of effective implementation of economic reforms" (p. 434).

Prime Minister Indira Gandhi in splitting the Congress party in 1969 specifically rejected the accommodationist approach.. By this point, as Frankel reviews in great detail in separate chapters, there had been failures of plan implementation, attacks on the socialist principles of planning, continuing domestic constraints and foreign pressures, and crises involving political stability. Political language changed from that of conciliation to that of conflict as Mrs. Gandhi allied with leftist forces and seemed to be engaged in an effort to what Frankel terms the radicalization of Indian politics.

Intermediary relationships between the leadership and the populace appeared to be broken with the 1971 Indira wave that promised to eliminate poverty. An overwhelming majority in Parliament and personal power combined in what some commentators saw as a political version of the goddess

Durga. Prime Minister Gandhi's power, nonetheless, did not result in basic changes despite a spate of new programs.

Radicalization was no more successful for Mrs. Gandhi than the previous accommodationist approach. Economic conditions deteriorated while internal Congress factions applied contrary pressures, and opposition elements became increasingly militant. Her ringing election promises of 1971 and 1972 failed "under conditions of an organizational vacuum in the states and ideological polarization at the Center, both within the ruling party and between the government and the opposition. . ." (p. 490).

Frankel also deals with the June 1975 Emergency and its subsequent termination in March 1977. Her narrative and analysis does not encompass the Janata party period, nor the subsequent return to power of Mrs. Gandhi in January 1980. Nonetheless, Frankel's basic argument, as summarized in her final chapter, remains valid.

First, Frankel concludes, radical agrarian reform is essential so as to change the pattern of economic concentration in the rural sector. Secondly, non-political changes at the village level are not sufficient as agents of change. Cooperative and *panchayat* institutions are captured by the rural elites. Flowing from these two conclusions is her major operational point. Peasants must organize or be organized in class-based associations if the rural poor are to become politically effective (p. 548 f.).

In her notable book on *India's Green Revolution,* Frankel brought into bold relief the political costs that result. In this equally notable volume, she effectively sets forth the need for more effective political institutionalization in India. In contrast to the past, she emphasizes that it must reach down to the rural poor rather than just to the rural elite. Her conclusions are not necessarily new. Nonetheless, the quality of the presentation will establish this as a standard book on India's political economy.

PAUL WALLACE

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Mao's People: Sixteen Portraits of Life in Revolutionary China. By B. Michael Frolic. (Cambridge, Mass.: Harvard University Press, 1980. Pp. 278. $15.00.)

While interviewing Chinese refugees in Hong Kong in 1975, a moment of ultra-left policy surges in China, B. Michael Frolic developed an image of daily life in the People's Republic of China which differed fundamentally from that painted by short-term travelers. These travelers had been describing a crime-free China whose young people and old leaders happily and selflessly served peasants and workers in a climate of economic equality and popular, political participation. Frolic's people, in stark contrast, sketched in a rape-plagued China where citizens lived in fear of theft by strangers and uprooted youngsters escaping the countryside, a China where bribes were so frequent they were considered normal, not corrupt. Frolic's people in 1975 portrayed a China whose military and civilian officials lived in privilege and power with an unbridgeable chasm separating them from frightened, ordinary folk who lacked connections or power.

By 1979, Frolic confidently revised and condensed 16 refugee interviews into *Mao's People.* He pointed out that official accounts from China now supported the painful reports of his people and contradicted the happier tales of earlier travelers. Frolic is correct. After the October 1976 arrest of ultra-left leaders, a controversial attempt was initiated on high in China to confront honestly problems of the dark side of China's polity. For a most vivid, readable and useful introduction to vital aspects of pre-1976 daily life in China, Frolic's people, *Mao's People,* can be highly recommended.

But it is not a scholarly volume. It has little to teach the professional analyst of Chinese politics. In fact, any of the scores of foreign researchers in China or even readers of the Chinese press could by 1980 discover more and better information on all the major topics discussed by Frolic's people.

Of course, the book makes little scholarly pretense. It does not even have an index. Its 60 pages of introduction, commentary and notes are often too simple or even misleading. Frolic implies that "Let me talk" (*Wo lai tan ba*) means "timorously" (p. 163). He grossly overstates the number of full-time agricultural workers (p. 242). Frolic underplays the role of all but Li Zhengtian in the Li Yizhe group (p. 257 f). His annotation of names and events is superficial (e.g., p. 17, the 5-16 group; p. 270, Wang Jinxi). He is wrong that "Chinese oil is an area that has remained above politics" (p. 59). One has to know about the Socialist Education Campaign to understand the references to 1964 (pp. 26, 165, 187). In short, only a teacher who really knows Chinese politics can get maximum mileage out of some of the insightful comments, asides and anecdotes sharply recounted by Frolic's people.

A non-specialist, as popular reviews indicate, confuses learning catchy nicknames from Frolic's people with getting inside the real guts of Chinese politics. The first two tales, for example, are full of delightful memories of raising more and bigger pigs. The reading is great fun. But the serious politics is omitted. Nothing tells the non-specialist why, after the North China Agricultural Conference of 1970, an all-out drive to raise pigs was so much a major policy opposed by the ultra left that in 1976, Jiang Qing, Mao's wife, on seeing a new, large, village pigsty, literally shouted out the charge that it was a Deng Xiaoping village.

Frolic's people usually offer daily gossip, not political analysis. Frolic intentionally "avoid[ed] asking political questions" (p. 3). He also avoided adding sufficient political context to make the personal accounts politically meaningful.

Naturally, Frolic's people lie. He doesn't put in the time to sort out truth from fiction (p. 64). Only once, by accident (pp. 215-16), with regard to a vicious criminal, who presented himself as a victim of the system, is a lie discovered.

Everyone distorts. But some of Frolic's people's lies were so stark and self-serving that I could not help but conclude that Frolic did not fulfill his scholarly duty of setting the record straight. It is misleading simply to record, as Frolic does, two rosy accounts by Han refugees of inordinately favorable Han treatment of Chinese ethnic minorities, Li and Tibetan, especially during the Cultural Revolution. In fact, at that time traditional cultures were treated by Red Guards as feudal remnants requiring destruction. While it may not be surprising that Frolic's people understate the mayhem they perpetrated or overstate their loyalty to Mao, these distortions cry out for scholarly correction.

Most misleading is Frolic's ahistorical claim that one villager's life "story is fairly typical" (p. 23), that Frolic's 16 people are a representative "cross section" of China (p. 5). One never meets the tens of millions of peasants who support the privileged military and distrust the campaign to end its special privileges. Those peasants believe they benefit from military privilege. One only gets a hint (p. 142) of the military people who consider themselves the guardians of the revolution, people who could act to defend themselves and their revolution against Party democrats.

People change as policy and possibility change. One does not meet those who come out of the Cultural Revolution decade committed to science, democracy and markets or those committed to re-entrenching themselves against further shake-ups. There is far more in China, including continuing political struggles over its fate and future, than Frolic's low-level people can tell.

Too much is claimed for Frolic's people. But that does not stop *Mao's People* from being a uniquely important supplementary text for those beginning to study Chinese society. If it were an inexpensive paperback, I'd strongly recommend *Mao's People* for course adoption.

EDWARD FRIEDMAN

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Jawaharlal Nehru: A Biography: Vol. 1, **1889-1947;** Vol. 2, **1947-1956.** By Sarvepalli Gopal. (Cambridge, Mass.: Harvard University Press, 1976, 1979. Pp. 398. $17.50; Pp. 346. $20.00.)

Jawaharlal Nehru was among the great Asian nationalist leaders and statesmen of the postwar era. His public career spanned almost half a century and since his death in May 1964, the Nehru literature has begun to grow rapidly. Nehru's letters, articles and statements up to the year 1936 now being published by the Nehru Memorial Fund have already reached six volumes. The secondary literature has also begun to proliferate as scholars, academics, journalists and political contemporaries both in India and abroad begin to attempt to assess Nehru's impact on India and the world.

Sarvepalli Gopal, a historian, former Director of the Historical Division of the Ministry of External Affairs from 1954 to 1966 and now professor of Contemporary History at Jawaharlal Nehru University, is in the process of completing a three-volume biography of Nehru and his times. The first two volumes, covering the period up to 1956, deal with Nehru's gradual rise to prominence in the freedom movement and his initial nine years as independent India's first prime minister.

Gopal's biography is built around Nehru's letters, diaries and notebooks, the earlier ones lodged in The Nehru Memorial Museum and Library in New Delhi, those written after September 1946 in the custody of Indira Gandhi, Nehru's daughter. Gopal has combined his use of the Nehru papers with a careful review of archives in India and Great Britain as well as interviews with many of the surviving participants in the events of the period. The result is a significant contribution to our understanding of Nehru and modern India.

Gopal is extremely sympathetic to the hero of his youth. Although Nehru emerges as a thaumaturgic personality who runs the country almost singlehandedly, we learn very little about his personal life. Moreover, except for Gandhi and Motilal Nehru, Jawaharlal's father, almost all of Nehru's close associates appear to either tarnish his image or undermine his personal authority. Key Congress leaders are out of tune with the general Nehru line and receive little attention, while close personal associates prove to be inferior people who function in a conspiratorial fashion. Krishna Menon is pictured as a deranged personality; M. O. Mathai, Nehru's private secretary, is seen as a usurper, disloyal, avaricious and opportunistic; and Rafi Kidwai is simply a manipulator who provided unsteady assistance rather than solid support.

Although Gopal's biography is extremely well written, it fails to develop a set of common themes. In an attempt to avoid being too eulogistic, the author chooses to focus on the historical and impersonal forces at work in India and the world. The reuslt is a sketchy biography which lacks human content. It becomes more history than biography. We learn a great deal that is new about Nehru's political ascendency, his role in Uttar Pradesh Congress politics and his relationship to Gandhi. We learn very little of Nehru as a human being. The units of analysis are a series of major historical events from the Cripps Mission to Kashmir, Suez and Hungary. These events are reconstructed primarily from the flow of Nehru's letters and correspondence. The result is a discrete set of detailed shapshots with few links between them and little relation to a larger policy process.

The picture of Nehru as prime minister of India that emerges from these two volumes is incomplete: Nehru's major contributions are seen largely in foreign policy terms. Gopal's biography reflects a view of someone sitting in the External Affairs Ministry and not in the prime minister's secretariat. At least two-thirds of the second volume deals with foreign affairs or domestic issues such as problems in Kashmir or Goa which had a strong international dimension. Thus despite the work's skewed focus, this very preoccupation with Indian foreign policy will make Gopal's *Nehru* essential reading for those interested in the early foundations of Indian foreign policy. Nehru is credited with helping to secure settlements in Korea and Indochina, enabling the British and French to redress their mistakes in Suez and doing his best in the circumstances for the people of Hungary. Any errors which might have been committed in dealing with the Hungarian crisis are attributed to a deranged Krishna Menon, Nehru's U.N. representative.

All criticisms of Nehru's handling of foreign affairs are dismissed as unfair or untrue. Nehru did not, in Gopal's view, concentrate on foreign affairs to the detriment of domestic needs, he did not treat India's neighbors differently from other more distant areas and he was perfectly "clear-eyed" about the Chinese leadership and the innate potential for a clash with China. He simply did not expect it in the immediate future. As for the absence of equidistance in Nehru's nonalignment between East and West, it was the Western powers and their attitudes toward colonialism, racism and their willingness to contemplate a general war that were at fault, according to Gopal. In particular, because of their great economic and military strength, the Americans tended to be overbearing and treated India not as an equal but as a country to be humored. This American attitude created a distinct prejudice against the United States in Nehru's mind.

Although Gopal credits Nehru with a wide variety of domestic achievements, domestic affairs do not get the treatment they deserve. There is almost no discussion of the formulation of India's Industrial Policy Resolutions, the creation of the Planning Commission, the first and second five-year plans, Nehru's role in the Congress party, the Nehru-Patel relationship, and the creation and development of the Indian political system and its key institutions.

Gopal's narrative ends in 1956, the "summer solstice" of Nehru's prime ministership. There existed, he argues, a sense of fulfillment in the air and confidence in India's future destiny. Individual freedom, social justice, popular participation, planned development, national self-reliance and a posture of self respect in international affairs were steadily being achieved under the guidance of a prime minister brimming with self-confidence.

Although imperfectly balanced, Gopal's *Nehru* is the most significant biography of the late prime minister to appear since his death. It contains an enormous amount of new information and is essential to understandingg the foundations of India's foreign policy. The biography will be valuable to both political leaders and scholars, and one looks forward with great anticipation to the concluding volume.

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The Cruel Dilemmas of Development: Twentieth-Century Brazil. By Sylvia Ann Hewlett. (New York: Basic Books, 1980. Pp. x + 243. $15.00.)

In form this book is a study in the political economy of development, using Brazil as an illustrative case. It is written by an economist who attempts to link her economic analysis to the central political issues of development and modernization. Given Sylvia Hewlett's approach to Brazil, this book, from the point of view of political analysis, fits into the general theme of "political models" of development. Brazil since 1964 is viewed as an example of a harsh authoritarian political model. This model is presented as the all but inevitable result of the regime's economic growth strategy within the national and international context of the time. Specifically, it emerged from a strategy of industrialization within the context of delayed dependent development—a situation characterized by certain "cruel dilemmas" which severely limit policy options. In brief, these dilemmas involve the inescapable political and economic costs of the growth strategy and particularly the maintenance of a system of state-structured and state-guided capitalism. In this mode of modified market economy the costs must fall on labor, the rural underclass and the growing mass of urban marginals. Authoritarianism is necessary to enforce the essential underlying pattern of distribution, and therefore civil and political liberties become another at least short-term cost or tradeoff of the approach. Most of the book is aimed at demonstrating this central point, and it documents the harsh economic and political consequences of the Brazilian model for the bulk of the population.

Overall, this work does not break any new theoretical or empirical ground. Basically it is a work of synthesis, both in terms of the theoretical perspective and the materials on Brazil. Hewlett begins from the well-established point that there are no easy ways to development and that all known routes involve costs and tradeoffs. The costs are determined by the nature of underdevelopment and the international economic order. The tradeoffs are determined by previous national patterns of political economy and strategic choices such as use of the market, role of international investment, industrialization versus agricultural development, capital-intensive versus labor-intensive industries, etc. Finally, certain global ideological choices—capitalism versus socialism—will have certain implications for emphasis on tradeoffs of social welfare values (capitalist Brazil) or of formal political freedoms (socialist China). The theoretical part of the study is a valuable synthesis of existing thought but it is a bit overly concise. Moreover, the study does not really dig deeply into the emerging body of literature on state society models in Latin America; nor does Hewlett probe the considerable debates on the relationship between economic states and political forms spurred in part by O'Donnell's hypothesis regarding the deepening of capitalism and modern authoritarianism.

The case-specific material is also mainly a synthesis of existing work. However, this material is handled quite well. Hewlett makes a great deal of economic information and analysis intelligible to the political scientist and she shows a sensitivity for the ways in which political and economic factors interact. The case study provides a compact introduction to the main issues of political economy in contemporary Brazil.

In the main, the theoretical and case-specific sections are well integrated and argued, with the exception of the concluding chapter which attempts to draw some comparative conclusions regarding Brazil and China. The book is well written and concise and should prove interesting and useful to social scientists interested in development, Latin America and Brazil. The major strong point of the book, however, is that it will heighten the political scientist's knowledge of and sensitivity to crucial aspects of development economics.

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African Upheavals Since Independence. By Grace Stuart Ibingira. (Boulder, Colo.: Westview Press, 1980. Pp. xvii + 349. $22.50.)

Grace Ibingira was a cabinet minister in Uganda until 1966 when he became a political prisoner, as a result of Obote's changes in the government. It was thought that Ibingira would oppose them. In 1972, he was released and served as ambassador to the United Nations until 1974. It is with this background that he analyzes the circumstances leading to the coups in Uganda, Ghana and Nigeria.

Ibingira compares them and then traces analytically first the effects of the colonial legacy on the governmental processes, and second the consequences of winner-take-all politics on each of these systems. Some interesting information is brought together about coups all over Black Africa from 1963 to 1978, with tables about imports and exports from the 1950s to the 1970s.

From these observations, Ibingira then concludes with some basic principles that should be included in the governmental process, including an ideological premise for government; leadership that accepts adversity and the idea of responsible opposition; constructing a sound, viable constitution; guaranteeing basic human rights; providing for "catch-up" programs in backward areas; an emphasis on common interests, promoting meaningful regional cooperation, and developing an enlightened foreign policy.

One should keep in mind Ibingira's own experiences as one reads these evaluations of the coups in his country, and in Ghana and Nigeria. The volume does share some useful insights on political development in the new African states.

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Political Crime in Europe: A Comparative Study of France, Germany and England. By Barton L. Ingraham. (Berkeley: University of California Press, 1979. Pp. xiii + 380. $22.50.)

History has spared few polities what has been traditionally viewed as political criminality. Few acts have not been treated as criminal by some governing authority. The identification of political crime—irrespective, perhaps, of its cultural, political or historical setting—is the task that Barton Ingraham has set for himself, among others. Whether it is enough to say that it comprises acts which officials *treat* as political and criminal, is arguable, granted that this may well be the only view that withstands critical examination. What has been done quite effectively, at any event, is the marshaling of much of the relevant historical, legal, and sociological data taken from the three political cultures of which Ingraham writes, and the inspiring of sensitivity to a few inferences and propositions concerning political crime as it exists within the theoretical criminal law model that each culture retains. Historical scholarship, for example, provides us with an awareness that pre-Enlightenment doctrine stressed the need for personal fealty. Political crime was initially regarded as a breach of faith and allegiance owed to a personal monarch or lord rather than political opposition on ideological grounds to an institutional order or to a regime. With the approach of the French Revolution, however, some modification of the severity of penalties may be discerned with detention and banishment coming into use, and death as a penalty being seldom invoked. Though England provided the model for liberalism and constitutionalism, change, once commenced in France and even Germany, moved at a more rapid pace. In all three countries, the concept of the state was given new meaning with the growth of administrative government and bureaucracy, forcing recognition of separate aspects of political power with competing claims on the loyalty of the monarch's subjects.

The revolutions of 1830 provide an understandable springboard for what Ingraham calls the springtime of the policy of leniency. The doctrine that political offenders should be dealt with politically, not in accordance with the strict provisions of the law, but with a leniency designed to conciliate political differences between governments and their opponents is seen to have emerged in the third and fourth decades of the nineteenth century. Specific statutory measures, enacted to protect the political criminal from arbitrary prosecution by government and to alleviate the punishments prescribed for "political offenses," bear this out in France. In Great Britain, sedition undergoes redefinition. Even in Germany, where aristocratic-bourgeois governments long held sway, liberalism gains acceptance, with political dissent verging on respectability. A shifting perception of politically deviant behavior underlay all these changes; doubt and ambiguity invest its once "evil" nature. With the revolutions of 1848 and up to the time of the First World War's outbreak, the view of the political criminal as a man of honor whose crimes are prohibited and punished on political rather than moral grounds reaches its greatest degree of acceptance, even in official circles. Slowly, however, moral reprehensibility presses for reacknowledgement, accompanied by concomitant punitive measures. The failure of revolutionary mass movements was to play no small part, as was the growth of nationalism, marked by a shift of attention to *external* political crime like espionage and subversion. Increasingly, the populations of all three countries coalesced in support of their governments. The purely domestic radical began to be perceived as a betrayer and weakener of national unity rather than the patriotic harbinger of needed social change.

The last great phase of which Ingraham writes is a so-called Age of Ideology, characterized by a withering of the policy of leniency. Between 1914 and 1970 conventional political crime begins to lose its aura of respectability and to become equated with external political crime; there is a merging of the offense against authority with the crime of betrayal. Nationalism's growth, increasing democratization, Russia's revolution, and positivism's enhanced standing, all play a part. The statist and racist ideology of fascism completely repudiated liberalism and this was enough to lend weight to the positivist view that political behavior might necessitate social defense against social danger. The differential treatment of political offenders—meaning preferential treatment in the nineteenth century—had, after all, been perverted under totalitarianism, had it not? This basically negative view of political crime in the twentieth century is held out as ensconced; it is the author's view that the fundamental split in ideologies between East and West makes it unlikely that nineteenth-century liberal perceptions will return. It is there that his case rests, impressively supported throughout by a vast welter of authority diverse in nature as much as are the social disciplines that they represent. Pound, Durkheim, Parsons, Mosca, Kirchheimer, Guizot, Bentham, Beccaria, Dicey, Lombroso, Sorokin and Weber are but a few of the seminal thinkers repaired to, noted at the risk of giving offense to the many of more current vintage referred to. It is there perhaps that something of the argument tends to founder, however, given the absence of a more overarching theme than is provided on so essentially cosmic and transcendent a subject as the one embraced. There are a number of melodies, and each tends to be well defined. There is, for example, the one that contends that there is inevitably a conflict between measures taken by governments in self-defense against illegal political opposition and the principles on which the liberal legal order is based. There is also one that argues that the limited use of legal repression for defensive purposes has not led to the growth of a "police state," but rather it has been the failure to take such measures during periods of weakness that has led to the polarization of factions within society and the defeat of democracy.

It is, perhaps, the theme that the political philosophy shared by most members of the government, including the legal fraternity, that limits and shapes the legal measures adopted to meet the political exigencies of the age that Ingraham seeks most to underscore. Supported by a copious amount of documentation, this is no small insight. It is, however, in the realm of prophecy that some feeling of frustration tends to follow. Granted that ideology is characteristic of our age, it would be nice to get some sense of the direction in which we are going, tentative and philosophic as such prognostication might well be. It would be nice to know what will become of ideas if not for people like Ramsay Clark, or Philip Agee, or Barbara Timm, or Andrei Sakharov. It would have been well to speculate on the future, if any, of milestones like the Helsinki Accords and SALT II. That a venture into international politics would necessarily follow is inescapably true, but better even a slight misadventure than a form of obliviousness. It is, especially since the Second World War, that a discipline of international relations has come into being, and this kind of work suggests the essential inextricability of the domestic with the international. There is little to suggest, for example, that domestic inflation and unemployment are unrelated to the perception of Armageddon's threat. Election-year politics certainly makes this a valid question, at any event. The confluence of the disciplines becomes apparent too in these contexts. Tall ordering this may be, but there is nothing inconsistent about this kind of suggestion and the fundamental ambitiousness of Ingraham's work.

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Traditional Authority, Islam, and Rebellion: A Study of Indonesian Political Behavior. By Karl D. Jackson. (Berkeley: University of California Press, 1980. Pp. xxiv + 375. $20.00.)

Through historical study of village-level participation in the Dar'ul Islam rebellion in West Java and a survey of Sundanese villagers in 1969, Karl Jackson seeks to establish that "traditional authority relations" rather than ideology, *aliran,* media exposure, socialization into national symbols, economic deprivation or physical isolation, are the key to Indonesian peasant political behavior. Primary and secondary historical sources pertaining to the rebellion are cogently synthesized in the opening chapter, and an appendix contains translations of several documents, including a draft constitution for an Indonesian Islamic state. The final chapters are devoted to more theoretical concerns. The central concept of traditional authority is defined and differentiated from more material, opportunistic, and short-term patron-client relations. The conclusion that traditional authority and physical coercion are the most effective means of political integration among the Sundanese becomes the basis for a dichotomous (traditional-modern) typology of political integration. In their mechanisms of integration, societies will "vary substantially within the context created by (1) the modernization process and (2) the organizational modes chosen by elites and counter-elites" (p. 278).

Jackson postulates the generality of traditional authority bonds from the vast majority of sample respondents who perceived themselves in both roles of advisor-patron and advisee-client and reported weekly consultation and that some of these bonds had endured for more than 25 years. Combining respondents' reputed importance in village affairs with the number of clients indicated narrow leadership pyramids in each of three primary villages. Readers may be unconvinced, however, since 84 percent of the sample was composed of members of the village elite and most respondents admitted to multiple advisors instead of a single, omniscient figure; however, Jackson denies that this could have been a source of pluralism and thus could have been problematic for its implication of advisee choice.

Second, traditional authority bonds are said to grow out of advisor-advisee relationships because of expressed feelings of moral obligation and the lofty father-child (*bapak-anak buah*) relationship toward them. Jackson infers a *paramilitary* mobilizational potential from respondents' assertions about their capabilities of obtaining help from advisees when in danger. The connection between insurrection and traditional authority "is most clearly displayed" by responses to a fictitious, "projective" essay about "a disturbance in the region"; 42 percent felt that a man should without deliberation go to the aid of his ex-commander to whom he also has a financial obligation.

Third, despite "structural isomorphism" in the politics of the three villages, Jackson found in the former Dar'ul Islam villages greater centralization of respondents' advice-seeking about extra-village problems, higher support for the idea of an Islamic state and, among only top village leaders (N = 20), greater expressed willingness to kill for religion. Because top leaders in both pro-government and Dar'ul Islam villages were found similar in social background, political knowledge, and religious beliefs, Jackson infers that traditional authority structures binding top leaders to political movements beyond the village rather than intra-village factors best explain the above differences between Dar'ul Islam and pro-government villages. Yet this argument seems contradicted by his findings regarding differences between top leaders in their attitudes toward an Islamic state, their willingness to kill, and their age and occupation. Finally, Jackson presents historical sketches on each village totaling 12 pages to establish definitively that traditional authority relations "dictated the character and content of national integration and political participation" (p. 276).

The problem of drawing large inferences from weak, indirect and contradictory evidence aside, fundamental problems in the methodology and research design increase my doubts about the *reliability* of the findings. The usual difficulty of establishing causation is compounded here by the sensitivity of insurrection and religious fanaticism in interviews, the seven or more years that elapsed between the end of the rebellion and field research and the meager resources available for the research. Even if one overlooks the small sample sizes and some arbitrariness in selection of the initial 19 villages, in light of unknown and difficult field conditions, the procedures used to select the subsample of 3 villages from which the main sample of 200 villagers was drawn likely resulted in considerable reduction of variation and hence a bias against alternative hypotheses. The inclusion of only 8 percent of the villages' total population in the strata for sampling and the heavy elite sample (from 79 to 84 percent) disregards the importance of careful stratification with small samples. Jackson employs questionable techniques for statistically gauging the effects of village structure on individual behavior and uses weak bivariate techniques with alternating variables and an occasional three-way table instead of multivariate techniques to evaluate the relative influence of competing/alternative independent variables.

Jackson's flare for measurement and for questioning most past explanations of political behavior in the literature on modern Indonesia make this a suggestive work, if a problematic one. It is likely to be of greatest interest to students of the Dar'ul Islam and those contemplating survey research in rural West Java. Those with more general interests may benefit from the discussion of the distinctive style of supra-subordinate relations among the Sundanese.

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Socialism in Provence 1871-1914: A Study in the Origins of the Modern French Left. By Tony Judt. (New York: Cambridge University Press, 1979. Pp. xiv + 370. $42.50, cloth; $12.95, paper.)

British Socialists: The Journey from Fantasy to Politics. By Stanley Pierson. (Cambridge, Mass.: Harvard University Press, 1979. Pp. viii + 403. $17.50.)

The titles of the books here reviewed suggest a greater degree of parallel development and comparability than either their focus or approach provides. They both deal with the "peculiar trajectory of Marxism" (Judt, p. 310) the one in France, the other in Great Britain. Judt's study is a masterful synthesis of social and political history ("social history which ignores the state . . . necessarily ignores an important dimension of the life of even the most ordinary of ordinary men and women"), while Pierson offers an empathic intellectual history of "strange and difficult men" and of marginal movements. The difference in treatment is not solely owing to the fact that early French and British socialism walked a different path.

Judt's explanation why a fairly extreme form of Marxist-inspired socialism took root in the rural department of Var refutes the many reductionist (often ecologist) theses that have been advanced concerning peasant attitudes and especially those who like Marx and others have dismissed the specific political identity of the peasantry in France and elsewhere. What transformed the historical and economic characteristics of Provence into fertile soil for the socialist movement, Judt demonstrates, was the economic crisis of the last third of the nineteenth century. Out of that crisis emerged a need for the peasantry to shed the traditional and no longer meaningful authority of the radical-leaning village notables, and to acquire a more collective and participatory role in their own affairs. Socialism appealed to them in class terms as it did about the same time to the working-class Guesdists in the Nord. True, a Proudhonist undertow in all of French socialism made the link between distressed peasants, cooperative ideals and socialist politics easier to establish. Yet, instead of merely protesting in a negative, atavistic and anarchist fashion, the Provençal peasantry favored the intervention of the state deemed necessary to ensure their survival even at the price of some independence. This made the "urban socialism" of Guesde and of other Socialists even more applicable to the problems the peasants had to face.

Once a relationship between the rural communities and the socialist movement was established it grew into a tradition and acquired thereby the same characteristics of a "structural" feature of the region's social and political life as anti-clericalism had been earlier. The older and often described regional habits of intense sociability had undoubtedly helped provide favorable conditions for the rise of local socialist support. Once made part of the culture, organized left-wing politics became the new form of sociability, long after the socialist party, the SFIO, had lost much of its revolutionary character and impetus that had made it attractive to begin with.

In the three concluding chapters Judt seeks to determine which insights his regional study might hold for an understanding of French socialism in general and of its failure on a national scale. In doing so he undoubtedly makes a contribution to what he calls appropriately "our sense of the infinite incomprehensibility of France" (p. 248). But on this scale, his discussion, while still opening valuable new perspectives on a number of puzzling phenomena, does not have quite the same explanatory power as his regional study. One may follow Judt in his demonstration that the French peasantry exhibits some of the characteristics of a class. Yet his (admittedly tentative) attempt to link the politization of the French peasantry in the second half of the nineteenth century to some broader view of the relationship betweeen social class and political behavior, does not even explain fully the difference between left- and right-wing rural areas. Indeed, "under certain conditions," and obviously not only under those prevailing in the Var, the peasantry provided a favorable soil for the socialist (and later for the communist) movement (p. 279). But it does not become entirely clear what these conditions were in different parts of a country "whose political development, at the national level, so outstripped and outran its rate of social change" (p. 277).

A sober discussion of the weaknesses of the Left in France distinguishes between "views from within" and "views from without," reviewing and debunking, without any acerbity, the divergent arguments that are most frequently put forward in discussions of this kind. In Judt's opinion, a history of the Marxist Left in France (and in other countries of "Latin Europe") must recognize how central a place the peasantry and its problems have occupied in that history and to what extent they have determined both the strength and the weakness of modern socialism.

Some of the discussion dealing with the effect which the welding of part of the peasantry to the SFIO has had on both the peasantry and the party is quite brilliant. What had begun as a relationship born of a meeting between the revolutionary doctrines of the Socialists and the social discontent of the peasantry ossified into a common conservatism. But there are other and weightier causes for the socialists' conservatism and the communists' sectarianism which resulted in the failure of both. Judt himself is well aware that "a book which has taken as its subject one small department of France ventures into broader reflections at considerable risk" (p. 309). That Judt takes this risk even the reader who remains unconvinced by his arguments has no reason to regret.

In a study of early socialism in Britain one would not expect a discussion of socialist sympathies among farmers. But it is somewhat surprising that also the labor movement is all but absent from Stanley Pierson's book. His study proposes to clarify the dilemmas that arose from the struggle to relate Socialist ideology to political realities in Britain during approximately the same period covered by Judt's book. Why were the Socialists, he asks, unable to convert any part of the working class to their ideas? But the "political realities," indeed the social and economic structures of the era, hardly surface on the pages of a book which instead is almost exclusively concerned "with the individuals and groups calling themselves Socialists and, particularly with those who served as organizers and propagandists" (p. 3). For this reason the book's subtitle is somewhat misleading: it is a story of fantasies which never led into politics. The various forms of the Socialist ideology, divorced as they were from the labor movement, always dissolved into sectarianism and headlong escapes from reality.

In many ways the book appears to be an expanded and rewritten version of Pierson's earlier *Marxism and the Origins of British Socialism* (Cornell, 1973). What has been added, on the basis of new source material, are more detailed biographical data (informed in part by Erik Erikson's approach), more insights into the contrary pulls between competing groups, more sound and fury about futile intra-faction fights, but little new analysis.

If in France Marx was frequently tempered by Proudhon, the thinking of the British Socialists used Marxist analysis, as it were, only in passing and seldom without apologies. The foremost saints of the various socialist churches and chapels were utopian aesthetes such as William Morris, Carlyle, Ruskin, Nietzsche (again and again!), and of course, Christ. The spread of socialism among churchmen, both Anglican and nonconformist, and among the Scots and the Welsh who had reasons to feel colonially exploited, is discussed interestingly enough. There is, however, no adequate explanation of the spurious alignments of the various socialist movements during the First World War, here with chauvinism, there with pacifism. It is true that the prevailing confusion was the fault of the actors rather than the author. The postwar pro-Soviet sympathies of a small number of Socialists, Beatrice Webb among them, are characterized, probably correctly, as another fascination with a presumed "spiritual power" of communism, rather than as a political conversion. Earlier some of the socialist thinkers appear, at least in this study, as Leninists-cum-New Left *avant la lettre*; these marginal individuals, Victorians all and all socially estranged from the middle classes to which they belonged, dreaded the "trade-union consciousness" of the working class and, with only a few exceptions, frowned on any "permanent and intimate cooperation with the masses such as was urged by Marx and Engels" (p. 258, quoting from the *Social Democrat* of 1908).

As is well known, the only socialist group which came to terms with the British political system were the Fabians and they did so by losing their soul (or most of it) in the process. Pierson's discussions devoted to prominent leaders of the group are quite enlightening. But here too attention is focused on doctrine and personality rather than on explaining the ascendancy of the Fabians, which was responsible for the insertion of Clause IV in the present constitution of the Labour party.

Pierson concludes (p. 346) that "the Socialists . . . did much to awaken the lower classes politically." That may be true. But this book which shuns a discussion of the labor movement has not demonstrated this fact. And when Pierson expresses his belief that "their struggles (i.e., those of the Social Democrats, the Fabians and Ethical Socialists) still hold much to instruct those who seek the road to socialism in a new time" (p. 350), one is left to wonder. If lessons there are, are they not all negative?

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Comrades and Christians: Religion and Political Struggle in Communist Italy. By David I. Kertzer. (New York: Cambridge University Press, 1980. Pp. xxiii + 302. $24.95, cloth; $6.95, paper.)

The search for an explanation of the regular, if not spectacular increase in votes for the PCI in Italy since 1945, in spite of the battle waged by the Catholic Church and the parties it favors, has been a central concern among political observers of the *via italiana*. More than nine Italians out of ten are baptized Catholics but one out of three, and one out of two in the red belt, favor the PCI. *Comrades and Christians* seeks to understand how the major protagonists interact at the grass-root levels and how the people respond to the struggle. How does a hegemonic communist party maintain a neutral stance towards religion at the national level, while combating it in the towns and villages of the peninsula? How does religion fare in the political struggle in the midst of communist Italy?

This is a study of Bologna, a beguiling, though possibly an anomalous communist-administered city, and more specifically of an atypical *quartiere* of the town, an overwhelmingly working-class neighborhood located in its poorest area, with a stagnant population in spite of a proportionately strong immigration from the south (at least until 1972 when the basic research was completed). In Albora (a fictitious name) the PCI draws two-thirds of the vote, the left in excess of three-quarters and the DC barely one-sixth, and not more than 10 percent of the 9000 inhabitants attend mass with any regularity. David Kertzer, an anthropologist, spent one year as a participant observer in Albora, and in *Comrades and Christians* attempts to impart a sense of the people's daily lives. He offers an interesting study, supported by much information, well-penned vignettes, attentive description, careful analysis and discussion of theoretical issues.

Kertzer deals with the communist and Catholic worlds (and the secondary groups that support them), the policy of the former and the crusade of the latter, using national and local materials to introduce the main theme of the book, the impact of the "strategy of interpenetration of Party and popular social life" (p. 36). In Albora, membership in the party—and 1800 of 6200 adults are members—"is neither a sign of life devoted to proletarian struggle nor a sign of personal dissatisfaction or protest" but "a badge of social identity rather than an expression of political militancy" (p. 48).

The impact of the social pressures exercised by a dominant party is explained and illustrated in chapters describing the plight of heavily discriminated-against southerners and their conversion to the PCI, the neutralizing tactics used by the party against a short-lived Catholic *gruppetto* and against a poor man's storefront Catholic church which, in the wake of Vatican II, attempted to penetrate the area and threaten communist supremacy. Of special interest is Kertzer's discussion of the battle for ritual supremacy where he distinguishes between rites of passage and rites of community. In spite of the tensions created for communists and Catholics alike, rites of passage remain monopolized by the Church, while the party dominates community rites, having transformed the old Catholic *feste* into *feste dell'Unità*, and created its own sacred symbol, the *Resistenza*. If the party cannot openly oppose church rituals because of its national religious policy, it counterbalances them by its social supremacy. Having officially repudiated integralism, it pursues hegemony viewed as a process where, in varying degrees, "people are imbedded in the social networks, organizational structure and symbolic system of a political force that seeks their allegiance" (p. 255).

To ascertain the social bases of political allegiance, Kertzer spends in his conclusion more powder than is necessary "to counterbalance the excessively mentalist bent of the literature" (p. 238), notably the Almond cold-war social pathology model of communism and the "belly communism" theory of folk wisdom. He goes beyond the two-worlds, two-subcultures models looking for tensions and interpenetrations, focusing on the examination of local organizational policy and activity, people's norms and people's behavior.

The argument is convincingly made in favor of the study of rituals, symbols, and the importance of social dominance by one party; by choice Kertzer devotes little attention to the ideological determinants of the elite and people of Albora. This is unfortunate and might have been ascertained by a study of the *consiglio di quartiere* and the various party sections and militants, socialists included. One troublesome question is the self-fulfilling nature of the theory of social constraint in this particular environment where 80 percent of the people vote left. Possibly, ideology is a poor explanation for the vote, but in a diffused form, ideology could (and does) play a greater role in other mixed *quartieri* of Bologna or areas of Italy, as others have demonstrated. While Kertzer does not deny this, he tends to side-step the issue. He has nevertheless provided a solid study which helps fathom the complexities of Italian voting behavior while illustrating how the PCI operates at the grass-root level.

ROBERT H. EVANS

University of Virginia

Respectable Rebels: Middle Class Campaigns in Britain in the 1970s. Edited by Roger King and Neill Nugent. (New York: Holmes and Meier, 1980. Pp. 200. $30.00, cloth; $17.50, paper.)

This book invites comparison with *The Middle Class in Politics* (Teakfield, 1978) edited by John Garrard et al. The subject matter is similar and one of the editors of this book contributed one of the papers contained in the other book. Given the variety of approaches it includes and its effort to provide a bit of historical background, Garrard et al. is overall the better book. On the other hand, King and Nugent do provide more of the "feel" of politics and offer more for the student of pressure groups.

The stimulus behind both books was the increased willingness of various segments of the middle class in Britain to organize in the 1970s to protect their interests and, more importantly, to move outside the normal channels of party politics and to employ direct, almost disruptive tactics frowned upon by established pressure groups. This increased activism and militancy caused many observers to assert that the middle class was in revolt. Ascertaining the validity of this view is the underlying concern of both books.

King and Nugent focus primarily on the middle-class pressure groups that sprang up during the 1970s in Britain, providing an account of their activities and internal politics. Separate chapters discuss ratepayers groups, small business organizations, middle-class unions, the National Association for Freedom, and moral groups like the Festival of Light. Another chapter examines the relation between these groups and the political parties. While it would not be correct to say that these chapters are descriptive only, yet they are more reportorial than analytical. While the authors (four of the seven main chapters are by King or Nugent with the other three contributed by others) comment at times on the ability of the groups to influence policy, yet these chapters are not case studies of the policy process. Instead, the authors simply recount the events that seem to have stimulated the formation of the groups, the actions then taken, and the internal squabbles that frequently weakened the groups.

While one would not want the authors to waste time in arid disputes over definitions, their unwillingness or inability adequately to conceptualize the middle class is a problem. They doubtless are correct that the middle class is diverse and diffuse, but simply to leave it at that makes it difficult to provide any sensible answer to the question of whether the middle classes are in revolt. One of the editors' conclusions is that middle-class unionism has much greater solidity and permanence than do the other movements. But since the author of the chapter on unionism could provide no satisfactory definition of union—does one include professional associations, for example?—one hesitates to accept this idea without question. Interestingly, insofar as one can identify a phenomenon of middle-class unionism, its growth begins before the 1970s and thus differs from the other movements covered in this book in this way as well.

While the groups discussed have not been entirely uninfluential, they do seem to have been at the fringes of power so far as the policy process is concerned. Given their limited impact, one wonders whether they are worth this much attention, whatever their visibility. The justification lies not so much in the impact of the groups as in their relation to the parties and what this suggests about future developments in British politics. Growing unionism among the middle class is not necessarily a Labour party bonus. Since non-manual unions tend not to affiliate to the Labour party, there is a growing divergence between unions affiliated with the TUC and those affiliated with Labour such that only about half of the TUC's membership also belongs to Labour. Furthermore, in those non-manual unions that do join the Labour party, more than half of the members contract out of the political levy.

As for the Conservatives, while the party has had close connections with some of these groups—the National Association for Freedom, for example—yet many of them strongly have attacked the Conservatives, particularly when Edward Heath led the party, because they felt that the Conservatives were not adequately defending middle-class values and interests. Thus while the editors do not believe that the middle class as a self-conscious, unified group was in revolt in the 1970s, yet they do see its increased activism and militancy as part of the recent fluidity of British politics; neither party can count on automatic loyalty within this segment and must work at winning support; substantial shifts in middle-class votes from one election to the next are quite possible.

While a review should not spend time criticizing authors for failing to write the book that the reviewer would have preferred, I do find it strange that this book has so little on environmentalist groups, and says not one word about the long-running saga of opposition to a new airport in the London area. Attention to the latter issue probably would have revealed a good deal more about the political process and the distribution of power than do the accounts of several of the movements included in this book.

JORGEN S. RASMUSSEN

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Italy in Transition: Conflict and Consensus. Edited by Peter Lange and Sidney Tarrow. (Totowa, N.J.: Frank Cass, 1980. Pp. 186. $25.00.

To even the most casual observer, it is obvious that Italy has undergone tremendous political, economic, and social change since the end of World War II. But, as Peter Lange points out in his preface to this excellent set of essays which brings out both the conflict and consensus in Italian society, American scholars, with the exception of a very few specialists on Italy, have not had sufficient background to interpret these changes knowledgeably. American universities have placed relatively little emphasis on Italy and seldom has Italy been studied in a comparative framework. The result is that writers often have viewed Italy's economic, social, and political problems as unique, and because of their lack of background, they have tended to be unduly pessimistic about Italy's ability to weather crises. One of the aims of this volume is to show that Italy is a modern industrial nation and that its problems are similar to those of other advanced industrial democracies. However, the manner in which the political, social, and economic forces react with one another may occur only in Italy.

In the volume's concluding essay, Sidney Tarrow emphasizes how important it is to give a balanced interpretation to Italy's problems and points to the many encouraging developments that have taken place in Italy during the past 35 years. Among these are better living conditions for the great mass of the Italian people; increased educational opportunities for the average Italian; changing social attitudes which have resulted in greater freedoms, especially for women—divorce and birth control now are legal; lessening of barriers between classes; and the more effective role played by the trade unions. All these changes are illustrative of the democratization and modernization of Italy during the past three and a half decades.

The essays on Italian political parties are particularly valuable for the political scientist. Lange's discussion of the PCI's alliance strategy and of the Communist party's decision to play down this policy in 1977-1978 is a penetrating analysis. The essay by Pasquino on the Christian Democratic party, in which he discusses the changes that have taken place in the party during the past 35 years, the relationships between the Christian Democrats and the government, and the future of the party, contributes significantly to one's understanding of the strengths and weaknesses of the Christian Democratic party.

Hines' study of the Italian Socialist party is a lucid analysis of the pivotal role that the Socialists have played in Italian politics during the past two decades. Hines points out that the PSI has had an influence disproportionate to its size because "unless the Christian Democrat party were [to have gone] over the heads of the Socialists to work with the Communists . . . any coalition [formed in the 1960s and 1970s] had to enjoy Socialist support" (p. 133). Yet once the PSI formed a government with the Christian Democrats, the Communists accused the Socialists of abandoning their socialist principles. Because of the cooperation between the DC and PSI, the Socialists have lost support among left-wing voters and the PCI has been able to monopolize the opposition. The PSI never has solved its dilemma; as a result, it is "surviving, but as yet it is showing little signs of reviving" (p. 147).

Five years ago, Tarrow and Blackmer coedited a series of essays on Communism in Italy and France which have provided students of Eurocommunism with a wealth of valuable source material. This smaller volume on contemporary Italy should give the same sort of stimulation to those who are interested in Italian affairs and who believe that Italy's problems are similar, in many instances, to those of other Western industrialized nations and thus can best be understood if examined within a comparative framework.

MARGARET HAMILTON

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Conservative Party Politics. Edited by Zig Layton-Henry. (Atlantic Highlands, N.J.: Humanities Press, 1980. Pp. xvii + 286. $30.00.)

Perhaps because of their own reformist political inclinations, political scientists have investigated socialist and liberal political parties much more thoroughly than they have conservative ones. In recent years, however, this imbalance has been redressed to some extent. The British Conservative party, never as ignored by political scientists as other European conservative parties have been, is the subject of detailed scrutiny in *Conservative Party Politics.*

The book focuses on policy making and internal politics of the Conservative party in the post-World War II period, especially 1964-1978. Despite its publication date, the book devotes almost no attention to how the party regained office in the May 1979 general election or to its activities since then. Most of the contributors are well-known analysts of their particular topics, including Andrew Gamble (economic policy), Zig Layton-Henry (immigration), Vernon Bogdanor (devolution), Nevil Johnson (constitutional reform), David Butler and Michael Pinto-Duschinsky (social background of Conservative leaders), Patrick Seyd (factionalism), and Ivor Crewe and Bo Särlvik (public opinion). Newcomers Nigel Ashford and Martin Burch contribute essays on the European Community and party leadership styles, respectively. Conservative party professionals Chris Patten and Andrew Rowe write on the general problems of Conservative policy making in opposition and the specific problem of developing links to trade unionists.

Many of these essays take issue with the conventional notion that the Conservative party is strongly hierarchical in its internal power distribution. Instead, it seems that the Conservative party leader has to struggle in various areas to have his/her wishes followed by the front bench, the party in Parliament, and the constituency parties. This may simply be part of the well-documented tendency in recent British party politics for dissident groups to have influence on specific policies. An alternative hypothesis, however, is that party policy making in opposition is less hierarchical than in government. Burch's systematic, careful comparison of Edward Heath and Margaret Thatcher as party leaders indicates that the government/opposition distinction is an important one. In a complex analysis, Burch suggests several interrelated hypotheses to account for differences in policy influence, both in opposition and in government.

The major shortcoming of the book is that few of the articles are theoretically oriented. In addition to the Burch article, the only other strongly theoretical pieces are those by Johnson and Crewe-Särlvik. Johnson examines the philosophical tensions underlying recent Conservative thought on constitutional reform. He argues that once the Conservatives regain office, they are unlikely to pursue major reforms. The behavior of Mrs. Thatcher's government as of this writing indicates that he is correct. Crewe and Särlvik closely study the nature of British public opinion and its implications for alternative Conservative electoral strategies. They find three distinct attitude dimensions within the public—left-right socio-economic, populist-authoritarian, and racial. Although at times their argument goes beyond the data, they have some suggestive observations about how the Conservatives can appeal to complex public attitudes.

The other articles offer competent description of recent tendencies within the party, although some of them, such as those by Bogdanor and Gamble, are more insightful than others. Among the topics not covered are Northern Ireland, Rhodesia, and defense. With reference to Bogdanor's comments on Scottish devolution, Ulster is another issue which the Conservative party, like its opponents, prefers to ignore. Enhanced by useful appendices and bibliography, this book is rich in information, even though it often fails to place the information in broader perspective.

DONLEY T. STUDLAR

Centre College

A Comparative Approach to Policy Analysis: Health Care Policy in Four Nations. By Howard M. Leichter. (New York: Cambridge University Press, 1979. Pp. x + 326. $21.50, cloth; $7.50, paper.)

Howard Leichter has written an interesting book, scholarly in its methods and tone, wide-ranging in its topics and evidence, thoughtful in its treatment of how and why the governments of Japan, Germany, Great Britain and the Soviet Union came to have the health care programs and policies they do. But the long title is a clue to the dilemmas he faced in giving focus to the effort. On the one hand he addresses the issues of comparative policy analysis, offering in the first part of this book a review of the competing conceptions, differing approaches, and conflicting findings of comparative analysts. For this effort, health care policy is not central at all, just one of many possible applications of the mixed approach he recommends. On the other hand, he "applies" his method to four cases in comparative health politics. Between the two efforts there is a large gap, one which Leichter only partially bridges.

Cross-national policy studies, as Leichter well understands, provide quite different types of data and inference from which one might learn. Some advance the limits of what is possible for government to do and teach us various ways of regarding familiar problems. Others allow us to weigh more carefully the determinants of policy; they permit the separation of culturally specific determinants from more generic ones, a task impossible within a single national case. In addition, cross-national study is relevant, though far from decisive, in appraising the probable effects of health policy options put elsewhere in practice. Finally, comparative analysis helps identify the ways in which different institutional structures shape the politics played out within them. In each case, the analyst stretches his mind, learning what is conceivable or, more ambitiously, what is likely under specified conditions. The major intellectual dangers lie in a facile importation of "lessons" from abroad or, still worse, a "world series" stance that seeks the winning strategy for a particular policy problem. National institutions and policies, as Leichter illustrates, lie embedded in particular histories and cultures.

Happily, Leichter's book is alert to both the benefits and the dangers mentioned. But the structure of the book nonetheless left me somewhat disquieted. While he makes a reasonable case for mixing the most similar and most different designs for comparative studies, Leichter has chosen the most different approach. He is interested in the effects of various regimes on public policy and accordingly wanted data on polities as different as Germany, Great Britain, Russia, and Japan. But the rest of his regime hypotheses would have been better applied to a wider set of nations and a smaller number of hypothesized relationships. At the same time, Leichter is drawn to configurative studies, efforts to sort out how what he calls situational factors, environmental influences, cultural and structural features combine to produce policy results in particular cases. So we are given literate, sensible, and thought-provoking essays on the health care policies of four regimes.

What we do not get is a tightly interwoven book on how health care policies and politics illuminate particular generalizations about modern governments. The sequence goes the other way around: plausible claims about comparative politics are subject to illustrative support or attack on the basis of characteristics of widely different regimes and those regimes' approach to medical care. At the close, Leichter offers some interesting "lessons" one might draw for the United States, but they are at some remove from the details of likely American experience. For that he would have had to investigate more similar regimes.

Not startling in method or finding, Leichter's book is nonetheless worth reading, particularly for those whose familiarity with public policy elsewhere is at the level of budget outlays. It is further worth reading because Leichter has composed it in an unorthodox academic style; he has not published these findings in articles and indeed is unknown to the specialists in health politics. That has, in some ways, hurt his book, as with the omission of the scholarship of Odin Anderson (on the U.K., Sweden, and the U.S.) and Deborah Stone (on Germany). But in other respects this book is a surprising and welcome addition to the literature of comparative health politics.

T. R. MARMOR

Yale University

The Collapse of Welfare Reform: Political Institutions, Policy, and the Poor in Canada and the United States. By Christopher Leman. (Cambridge, Mass.: MIT Press, 1980. Pp. xviii + 292. $19.95.)

The intensive examination of similarities and differences among a small number of countries has become an increasingly popular research strategy for scholars in comparative public policy. Perhaps as a response to the findings and criticisms of the comparative American state policy research of the 1960s and early 1970s, researchers have recognized the need to explore in much greater detail the ways in which politics makes a difference for the making of public policy. Increasing numbers of books and articles are now appearing which explore such questions for a small number of governments. In *The Collapse of Welfare Reform,* Christopher Leman provides us with one of the very best examples of this sort of work. Focusing on welfare policy and welfare reform in Canada and the United States, Leman examines similarities and differences, and argues that the principal explanation for the differences between these two similar systems lies not in political culture or socioeconomic factors but rather in political structures.

Chapter 2 discusses historical and demographic factors in the growth of poverty policy, emphasizing particularly the relatively greater insularity from public antagonism toward welfare in Canada. Chapter 3 describes the very similar reform proposals in the two countries which laid the groundwork for debate on welfare policy and reform. Chapters 4 and 5 chronicle the resulting debates over these reform proposals in each country. The core argument for the explanation of differences between the two countries—differences in political institutions—appears in chapters 6 and 7.

The book has several strengths which, I believe, distance it from other similar comparative efforts. First, it discusses the problems of poverty in considerable detail and in a dispassionate manner so necessary for social analysis, while at the same time attempting to explain the emergence of poverty policy and policy reform. Systematic efforts to explain public policy often deal rather clumsily with the concept of "problems" in that policy area. Likewise, treatments of policy problems often lack the dispassionate perspective necessary for good scientific explanation of resulting public policies. Leman seems to combine these two perspectives successfully.

Second, the chronological discussions of the stages in welfare reform are both non-tedious and comprehensive, and are appropriately intermeshed with analysis of the significance of those events. Third, the book devotes considerable attention to the economic bases for arguments over welfare reform. The discussion of the negative income tax and its complexities with regard to income supplement and work incentive is one of the best I have seen—although it is presented in entirely non-technical terms. The detailed treatment of the problem of measuring poverty in two countries is equally well informed.

The principal weakness of the work is that it really has no broad theory which guides the research. The "framework" presented in the first chapter is not a theoretical framework but rather a statement that the impact of politics on similarities and differences between welfare programs in two economically similar nations will be explored. The analysis certainly remains true to this purpose. But, in the end, the components of the explanation become very familiar ones reminiscent of the 1950s preoccupation of political scientists with the virtues of responsible party government in a parliamentary system. While there is not much new, however, in the contribution to theory, there is great value here in what is provided on this older and very important question. Leman shows in comprehensive fashion that variation in political parties, legislatures, elite goals, bureaucracies, and prior public policies contribute systematically to the differences. For that reason and others, this book is one of the very best analyses of public policy differences between countries. It should be read with equal care by specialists in public policy, comparative politics, and American politics.

ANDREW T. COWART

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The Hare and the Tortoise: Clean Air Policies in the United States and Sweden. By Lennart J. Lundqvist. (Ann Arbor: University of Michigan Press, 1980. Pp. xii + 236. $15.00.)

Lennart Lundqvist has carefully analyzed the differences between the United States' 1970 amendments to the Clean Air Act and the Swedish Environment Protection Act of 1969. He concludes, as have such American analysts as Charles Jones, that policy makers for the "hare" (the U.S.), unencumbered by realistic considerations for technological or economic feasibility, chose a radical "technology-forcing" approach to pollution control. It set radical goals for the ambient air, centralized authority in the Environmental Protection Agency, provided for citizen participation in the enforcement process through lawsuits, and established unrealistic deadlines for the accomplishment of the goals. Swedish policy makers, concerned about unemployment and Swedish industry's competitive position in international markets, established no ambient air quality goals. Instead, the "tortoise" set up a franchise system through which individual industries bargained for permits from a centralized government agency (the Franchise Board for Environment Protection), from which process the public was excluded.

The reason for the different treatment by the "hare" and the "tortoise" was not a difference in the amount of pollutants in the air, nor even a difference in public perception of the problem, which Lundqvist describes as rising simultaneously in both countries in the late 1960s. It is rather owing to differences in the political structures, an appealing conclusion for those of us who wish to believe that the system does make a difference on policy outputs. Lundqvist argues that the Swedish parliamentary system, with its responsible parties and need to consider the possibility of taking over the administration of programs, forces individual politicians to be conservative in their approach to any new policy, as they must face the possibility of being held accountable for its failure. Ergo, the tortoise approach to public policy making. The individualistic politics of the United States, on the other hand, forces politicians such as Edmund Muskie to respond to the whims of the moment and formulate unenforceable policies. This makes for very plausible reading, especially when one considers the U.S.'s backing down on clean air depicted so graphically by Lundqvist after the problems of energy and the economy supplanted it in the public's mind in 1974.

Several problems remain unanswered. First is the characterization of U.S. air policy as a radical departure from the past and radically different from the Swedish. Neither is entirely accurate. Just as the Swedish clean air strategy came partly from modifications of its water pollution policy and partly from innovations made by the U.S., the hare's policy did not spring full-blown from the head of Zeus. It came after years of incrementalizing from the first Clean Air Act of 1956, which was in turn dependent on the water pollution control act begun in 1948. Admittedly, these two pieces of legislation contain assertions of public health goals and ambient air and water quality, but they are based, like the Swedish one, on an intricate system of guidelines for emissions control and individual permits that are negotiated in excruciating detail in the administrative process.

While the 1977 amendments did relieve industry of some of its responsibilities, it is important to remember the 1970 law's provisions for postponement of the automotive emissions deadlines as the cause of the first retreat on goals. Industry was not so asleep at the switch as it has been depicted, nor was the U.S. Congress so unrealistic or unresponsive to the traditional vested interests. Nor has all the hare's behavior since 1973 been strategic retreat. Prevention of significant deterioration of clean areas was added incrementally first by the courts, then EPA regulation, and finally ratified by Congress in 1977. While it is true that U.S. policies emphasize due process and the judicial role, it is also true that two can play at legalisms; industry has increasingly utilized its right to challenge administrative decisions in the courts. As Lundqvist rightly points out, U.S. policy has been less centralized than Swedish; and subnational politics has traditionally been more responsive to industry's economic arguments. A number of rival explanations exist for the hare's sluggishness in recent years other than a rapid response to changed public opinion (which, incidentally, polls indicate continues to support environmentalism).

American policy analysts, used to depicting the iron triangle of vested interest, congressional committee, and agency acquiescence as the example par excellence of bargaining, will be shocked to learn that Sweden is the true home of incrementalism. One wonders, given industry's stranglehold on the decision-making process portrayed by Lundqvist, why the Riksdag bothered to adopt a pollution control act at all. One further wonders how Sweden's progressive policies on such topics as social welfare, abortion, gun control, and taxation ever came to be. There is less mystery involved in the "steady progress" the tortoise has continued to make in air pollution; it must be attributable to the hare. If one's primary criterion for air pollution control is technological and economic feasibility, one clearly depends on someone else's (a technology forcer's?) development of the feasibility. This theory is given credence by Lundqvist's mention of the Swedish auto industry's response to American emission standards which in turn enabled the Swedish Pollution Control Board to adopt higher standards.

The representation of U.S. policy makers as eager respondents to every breeze of public opinion must also be questioned. Given the enormous public support for such policies as freedom of choice about abortion, as well as gun control, it is obvious that members of the U.S. Congress are bound much less by the pressure of majority opinion than they are by the threat of the organized few on crucial issues. Today's impasse on energy policy is a good example. Lundqvist describes the U.S. public as having been preoccupied with energy problems since 1973. Yet no radical departure from previous policy has been made, although innumerable suggestions have been offered. The iron triangle of policy making seems to be effectively representing the point of view of the major interest group (the energy industry). Yet environmentalists have been able to stalemate some of the most ecologically irresponsible programs also. The American system of gradualism (or even immobility) seems to be intact. Perhaps the Clean Air Act amendments of 1970 were an aberration that Lundqvist has mistaken as the United States' normal mode of policy making, or perhaps it was not such a tremendous departure from normality.

LETTIE M. WENNER

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The Politics of Social Security in Brazil. By James M. Malloy. (Pittsburgh: University of Pittsburgh Press, 1979. Pp. xii + 200. $13.95.)

James M. Malloy has written an insightful and interesting analysis of the relationship between the state and society in Brazil, as viewed through the evolution of the country's social insurance system. Classifying Brazil as a patrimonial state that experienced delayed dependent capitalist development, Malloy shows how social insurance policy was used by the bureaucratic-administrative elite to link "potentially powerful groups into dependency relationships that are controlled from the top down" (p. 16). More specifically, the social insurance system originated and, for most of its history, was used as a mechanism to coopt first organized labor and later, the organized rural population. Today the social insurance system includes more than 43 million rural and 45 million urban workers.

Malloy devotes most of his attention to the two periods when the social insurance system worked more or less as planned, that is, during the Vargas period (1930-1945) and the current period of military rule that began in 1964. Malloy persuasively argues that social policy toward labor was "launched when labor was at one of its weakest points organizationally" (p. 33) and as a result, did not have "the power to define the key components of the program aimed at its problems" (p. 49). The entire social insurance system was an attempt by the political elite to escape class conflict by dividing organized labor into numerous unequal and competing groups. Such preemptive incorporation of labor led logically to a dependent and easily manipulated labor movement.

The most fascinating section of Malloy's analysis, however, deals with the 1945-1964 populist period. During these years, the bureaucratic-administrative elite lost control of the patronage that the social insurance system provided. The state continued to grow in size and formal power; however, "its capacity to act effectively in a number of important policy areas actually declined," in great part because "the patronage system [had fallen] under the control of middle-level political and labor leaders who used it to build their own power bases and further their own careers" (p. 78). Again, in Malloy's words, "While the Vargas regime had created a system in which no single group or alliance of groups could dominate the state for its own ends, the very structures of state cooptation and control allowed particularistic groups to reverse the flow of the process and in turn coopt and control pieces of the state" (p. 82). Malloy attributes this development to the inherent weakness of the Brazilian state: "If, under Vargas, the unions were in a sense weak and coopted, so was the state; the weak, in effect, fed off and depended on the weak. The upshot was increasing rigidity and immobilization" (p. 90).

The events of 1964 reversed this trend. After the coup, independent unions were destroyed. In the countryside, independent rural organizations were replaced by a "vast array of state-sponsored and controlled worker and employer unions" (p. 131). A rural social protection scheme was instituted as part of a system for the cooptation and control of the rural population. To state it differently, the 1930-1945 system for the cooptation of urban labor now has been adapted to the countryside. Malloy ends his study on a pessimistic note: "In the rural area in particular, overall central control has grown and the capacity to mobilize broad-based autonomous action has diminished" (p. 133).

Is it possible that Malloy is unduly pessimistic and that his pessimism is the result of his overemphasis of the bureaucratic, authoritarian and corporatist aspects of the Brazilian political system? The 1945-1964 period was not, after all, an aberration. It was part of the political system itself. The facade of the Vargas regime was stronger and more authoritarian than its interior. The attempts of a weak state to coopt a growing labor movement had built-in limitations that became manifest after several decades. Perhaps the same process will repeat itself in the future, as leaders of the now-coopted peasant organizations gradually appropriate to themselves the state's power to distribute patronage and use that control to pursue goals that are different from those of the bureaucratic-administrative elite. Such behavior already characterizes sectors of the labor movement. The fact that peasants were initially organized and incorporated by a weakly institutionalized military regime makes it at least reasonable to anticipate future independent action by now-coopted rural groups.

SUSAN KAUFMAN PURCELL

University of California, Los Angeles

The October Revolution. By Roy A. Medvedev; trans. by George Saunders. (New York: Columbia University Press, 1979. Pp. xix + 240. $14.95.)

This very interesting volume deserves two reviews, one on its analyses of the Bolshevik Revolution and one on what it reflects in contemporary Russia. Roy Medvedev is a most prominent Soviet dissident. This is his latest effort at honestly discussing Russia's past in the face of the Soviet state's relentless determination to control the image of that past to make it legitimize the present.

A renegade to Soviet authorities, Medvedev consistently asserts he is a true Marxist-Leninist.

Medvedev's objective is to explain where Lenin and his revolution went wrong, which of course means dismissing the official view. He considers the possibility that the revolution was premature, as various socialists argued at the time, but rejects this too. A revolution must be made, not simply awaited, and Lenin was right to seize the opportunity to achieve social justice and end the war. He finds Lenin's genius to have been indispensable at that point.

While applauding their intentions and often realistic judgments, Medvedev finds Lenin and his colleagues held to certain pre-revolutionary views that were "premature, unrealistic, utopian, and simply wrong" (p. 89), leading to serious errors once they were in power. He criticizes the dissolution of the Constituent Assembly; the proper course was to postpone the elections until they could better reflect Bolshevik popularity. However, the gravest mistakes lay in the reordering of economic life. Medvedev attacks the abolition of private trading, the attempt to organize everyone in cooperatives and the wide-ranging nationalization of industry as unnecessary measures and beyond the fledgling state's capacity. Black markets sprang up, speculation flourished, industrial production shrank, and—lacking goods to acquire for their grain—the peasants withheld it.

At this point, Medvedev argues, the government should have been conciliatory, should have adopted a tax-in-kind system to encourage grain production, should have permitted the revival of trade, and should have curtailed nationalization. In 1921 such steps were the core of the New Economic Policy (NEP); Medvedev wishes they had been adopted three years earlier. He feels that, along with such policies, a coalition government with other socialist parties would have been appropriate.

Instead, the Bolsheviks used coercion. Workers' detachments and military units were used to seize grain, and the official use of terror spread. Committees of poor peasants, set up to assist grain confiscations, proceeded to seize the larger peasant landholdings in a second wave of land reform. These policies alienated the Bolsheviks from the masses, making bitter civil war almost inevitable. Lenin learned from this and introduced NEP, but Medvedev feels his successors have returned to the mistakes of 1918 and persistently mismanaged rural life at great cost. Thus he concludes, "It does not hurt to keep in mind certain aspects of NEP even today" (p. 187).

His discussions are clear and stimulating throughout. However, for Western scholars many of Medvedev's interpretations will be familiar or flawed. The Bolsheviks get too much credit for organization and political realism. Their rural policy is described as mistaken, but seems more nearly an expression of profound antipathy toward and ignorance about the greatest part of the populace, hardly the epitome of realism. Medvedev ignores the fact that NEP was adopted when the Bolsheviks were eliminating their last organized political rivals. He doesn't see those two steps as connected, but a NEP in 1918 would have meant a dialogue and even a coalition with other parties (as Medvedev suggests), contrary to the pattern of Lenin's entire political career and style. Lenin was tolerant in politics only when his power was unchallenged and unshared. In fact, nowhere in the book is there a sense that revolutionaries thirst for power independently of their ideological goals.

As a Marxist analysis, Medvedev's views also fall short. Engels, Plekhanov, and Trotsky (among others) feared the effects of a premature proletarian revolution. Post-1917 events often seemed to confirm their apprehensions, yet Medvedev rejects such views in unconvincing fashion. He refers instead to understandable mistakes and misjudgments amid excesses of revolutionary enthusiasm. Surely a Marxist analysis would trace mistakes and failures to larger forces, to the socioeconomic context in which they occurred.

But the book merits attention for the attitude it reflects, a commitment to honest inquiry so that Russians might learn from the past rather than be hostage to it. In Medvedev's account the revolution's heroes make mistakes, their opponents are not without insights and virtues, and the meaning of events is open to debate. Medvedev believes that only Soviet historians can assemble a true picture of the revolution. For ourselves this is dubious, but he is surely correct for his own society. Soviet historians must guide their fellow citizens in exploring the events and implications of that misshapen struggle. He is also correct in seeking to reaffirm the goals of 1917. In that direction lies the reformation of Soviet life and the true triumph of the Russian Revolution.

PATRICK M. MORGAN

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Patterns of Policy: Comparative and Longitudinal Studies of Population Events. Edited by John D. Montgomery, Harold D. Lasswell, and Joel S. Migdal. (New Brunswick, N.J.: Transaction Books, 1979. Pp. vi + 345. $16.95.)

It has become practically a ritual for book reviewers of edited volumes to comment on qualitative differences between chapters and lament the lack of a clearer overall focus or analytic framework. This edited volume, however, is qualitatively different overall. In fact, it is exactly the diversity of approaches to studying population policies which is the primary issue of inquiry in this volume.

As theoretically argued in the introduction and several of the chapters, policies affecting population changes cannot be understood unless more careful attention is given to the multivariate interplay of political, economic and cultural factors which have influenced population increases, decreases and movements. In order then to discern patterns of population policy, these authors argue that a more multidimensional analytic approach is needed. The approach suggested is seen as having three related dimensions. First, there are the "internal dynamics" of any particular population policy experience. Here the use of single-nation or comparative case studies of the decision-making actors and the sequences or circumstances leading these decision makers to act as they do on population policy matters is cited for analysis. Five such "internal dynamics" case study chapters, covering countries from Central America to South Asia, have been included in this volume.

The second related dimension is the "context" within which population policy issues arise—from the context of policy initiation through the context of policy implementation. As pointed out in several chapters, the policy "context" can vary dramatically at different stages of any particular policy process. Hence the actors that are important in the internal dynamics of policy initiation may become ineffective and less important than other actors or institutions at the stage of policy implementation. Such changing patterns within any particular policy process are seen as being influenced by four types of contextual elements. Differences in levels of economic and social development, differences in cultural values, differences in past policy decisions, and differences in political institutions are seen as interacting factors which must be studied comparatively in order to discern patterns of policy context upon population policies.

Several chapters analytically wrestle with this question of interacting contextual factors influencing the internal dynamics of state policy actors and institutions in terms of public and private decision making. Both "public policies" and "civic policies" (actions taken by individuals and families) are correctly seen as being susceptible to the classic problems of defining rational and irrational decision making and predicting intended versus unintended or indirect outcome effects. This question of how to distinguish analytically between public and private decision making in policy areas as personal as the issues of birth, marriage, residence, mobility, death, etc., is provocatively discussed, but certainly not permanently resolved. Particularly useful for comparative political scientists are some of the questions raised in several of the chapters about the problem of how to define more adequately the differences between public and private population policies. This is especially a problem for studies of developing countries where the transition from more private traditional decision-making units, such as family, clan, ethnic group, etc., to the more public processes of the laws and regulations of centralized state bureaucracies is still under way. Analytic reminders of this need to incorporate both public and private decision-making processes into any description of any particular population policy or broader statements about patterns of policy are indeed important. Far too many single-nation or comparative studies in the policy research field still erroneously assume that discursive reconstruction of the policy decision making of public institutions completely and adequately describes "policy" in whatever policy area is being studied. An argument made here by John Montgomery, Joel Migdal and others is that even "public policy" cannot be completely understood by limiting one's analysis to public actors, institutions and their decisions. Ideally, as political scientists, we should not need to remind ourselves that the power of public arenas is complexly limited by how private arenas perceive attempts to influence "personal" values and behavior. A clear challenge is raised here to define more clearly our conceptualization of "policy" itself—a task deserving more analytic attention.

This question of more adequately defining "policy" and the "public" and "private" aspects of policy links the first two dimensions of internal policy dynamics and policy context to the third and last dimension of "policy projection." As succinctly stated in the introduction, "The basic objective [of policy projection] is not forecasting; it is control." Control is said to mean "bringing as much rigor as possible into the process of forecasting despite the difficulty of forecasting social change." Three very different chapters offer thoughts on policy projection. Phillip Clark offers an analysis of how Australian immigration policies will become more flexible because of the interrelated nature of past policy decisions, current values within the Australian population, and future economic development needs. Gaither Bynum considers the impact of possible scientific and technological extensions of the human life span upon demographic patterns and future policy changes. Donald Warwick discusses the problems involved in conceptualizing and projecting the complex interactions of internal-national and external-transnational policy ("environment") inputs upon policy formulation, implementation and feedback.

Although separated into the other two volume sections on internal policy dynamics and policy context, the other chapters of the volume correctly attempt to integrate these two dimensions of analysis. John Montgomery analyzes hypothesized links between population policy and social change, discusses the problems inherent in using population policies as social experiments, and suggests that "better knowledge [of the links between policy and social change] will depend on the cooperative participation of [population] program planners and managers in observing policy experiments of the future." John Ickis examines the changing nature of organizational behavior and interest group coalition-building within the context of the personal, professional and political networks of El Salvadorean population politics since the early 1960s. David and Frances Korten demonstrate the importance of regional policy models in their analysis of the similarities of the Costa Rican and Nicaraguan family planning programs with the El Salvadorean program, despite major cultural, economic and political differences between these countries. Farrukh Iqbal examines evidence that land reform policy in Pakistan and elsewhere has an indirect short-run pronatal effect on population growth rates. In the long run, however, land reform will have an antinatal impact if instituted along with other rural reform policies effective in improving primary education, health and nutrition, and old age security. Myron Weiner discusses the impact of past and present ethnic prejudices, practices and privileges upon migration and employment in the state of Assam, India. Joel Migdal suggests that the unintended effects or "administrative countereffects, the indirect and contradictory effects of direct [migration] policies" in the Third World have occurred primarily because the capacities of centralized bureaucracies to carry through effectively with the implementation of their policies has not yet been developed adequately to replace "old political forces, . . . [who] have proved exceedingly resilient and adaptable." Nancy Lubin, in contrast, looks at some of the unintended problems which have resulted from excessive centralization of population policy in the Soviet Union. Unwilling to adapt their national Party pronatal population policy to regional cultural differences and unwilling to invest the necessary funds for more effective policies in the social need areas of better housing, day care, family allowances, and household convenience consumer goods, the Russians have become increasingly frustrated with continuing declines in their national population growth rate and undesired disproportional population increases among their Muslim "minorities" despite Party policy goals. Eleanor Glaessel-Brown argues that the seasonal labor migration patterns from Haiti to the Dominican Republic and the emigration of excess population from the Dominican Republic to the United States fills economic needs in all three nations while relieving pressures for any substantive socioeconomic change.

There are unfortunately some editing problems —a missing table in chapter 1 and a less than thorough index. Despite these minor inconveniences, the concepts broached in this volume should concern anyone interested in comparative development. Whether one's research focuses on one nation, a specialized aspect of demographic planning, or on broader themes of comparative social, economic or political analysis, it is becoming increasingly important to understand how and why population changes may or may not be effectively affected by national and transnational population policies.

ANELISSA LUCAS

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The Reluctant Party: Labour and the EEC, 1961-1975. By L. J. Robins. (Ormskirk, Great Britain: G.W. & A. Hesketh, 1979. Pp. xi + 155. £5.95, paper.

Throughout its four-score-year history the British Labour party has had in its ranks an important idealist faction dedicated to internationalism. However, for a long period this faction was dominated by middle-class leaders and the working-class representatives took little interest in their activities. Furthermore, to a significant degree, foreign policy issues generally were the concern of the political wing of the party rather than the industrial arm of the Labour movement. The trade union leaders and their representatives in the party were concerned with domestic bread-and-butter issues. The decision to have Great Britain join the European Common Market went against the traditional factional interest of the Labour party. What began as an apparent foreign policy issue was quickly perceived as being a major domestic concern and all parts of the party became preoccupied with it. The almost total interdependence of domestic and foreign policy points up the complexity of the issue.

This complexity caused serious difficulties for the Labour party and resulted in a confused unclear policy throughout the period in which the issue has been relevant. L. J. Robins appropriately entitles his book *The Reluctant Party* as he traces the debate about British EEC membership within the Labour party from the period of the decision of the Conservative government in 1961 to apply for membership until the referendum on the issue in June 1975.

A far too brief chapter on the decision-making process within the Labour party and a general discussion of Labour's foreign policy aims introduces the book. Robins then describes events and activities related to Labour and the EEC based on a chronological approach. There are several useful themes which recur throughout the book. These include the role of leadership in the party, the everpresent problem of party unity, party pressure groups and electoral considerations. Robins also writes of Labour orthodoxy in several chapters but he is less convincing on this topic because of the complicated nature of the issue and he does not develop a clear meaning of orthodoxy over time. He also endeavors with mixed success to discuss policy outcomes in terms of the themes mentioned above.

While offering little of originality for students of the British Labour party, the book is a useful review of the implications of party factionalism and the resulting intra-party strife in this federated, pluralistic reform party. Particularly noteworthy are the descriptions of the difficulties faced by the leaders of the Labour party in directing party policy and, at the same time, maintaining party unity. The author correctly sees the party as vacillating, inconsistent, and at times, lacking in any policy toward Western Europe. Leaders Gaitskell and Wilson epitomized the vacillation and even contradictory efforts in their quest to maintain unity. Also, at times their inconsistency was manifest because, rightly or wrongly, they perceived threats to their leadership positions on the issue of the Common Market. The weaknesses of Gaitskell as a leader in this area may surprise some readers.

Although the referendum on continuing membership in the EEC did not receive sufficient treatment or analysis as a precedent-setting political option, the author does perceive it as still another in the long series of instruments and compromises used to maintain party unity.

The book includes a full and useful bibliography. It is unfortunate that Robins confined himself to a narrow descriptive treatment of his subject. Comparisons with a few of the other major issues which have divided the Labour party in a concluding analytical chapter would have improved the book.

STEPHEN P. KOFF

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Nepal: Profile of a Himalayan Kingdom. By Leo E. Rose and John T. Scholz. (Boulder, Colo.: Westview Press, 1980. Pp. ix + 144. $16.50.)

This is the first in a series of short introductory volumes concerning the countries of Asia. Unfortunately, the limited format only allowed Leo Rose and John Scholz to scratch the surface of their subject and to raise many provocative issues which could not be explored in detail. Nevertheless, the result is a concise and lucid overview of the essential historical, cultural, economic, and political characteristics of Nepal.

There are several reigning monarchs in the world, although few of them actually rule. Nepal's ruler is one of the exceptions. The modern history of the country dates from the restoration of King Tribhuvan in 1950 after more than a century of oligarchical dictatorship under the Rana family, during which the form but not the substance of the monarchy was retained. The next 20 years were critical in the political and economic development of the country and Rose and Scholz have devoted one of their five chapters to it.

While Tribhuvan seemed to favor parliamentary democracy, his successor King Mahendra did not. He abolished political parties and issued a constitution which defined the monarchy as the central political institution. The authors point out that the absolute authority of the king is limited somewhat by traditional practices, especially from Hinduism. While King Birendra has further strengthened the monarchy, pressures for liberalization have increased which culminated in an outbreak of violence in early 1979. Birendra's response was to call for a referendum to let the people decide whether they wanted to continue with the existing system or to establish party government. The status quo was endorsed by a majority, but 45 percent favored the party government idea, indicating the need to increase participation in the political process.

The book is oriented toward political and public policy matters, which probably reflects the fact that Rose and Scholz are both political scientists. They note the importance of such social systems as the persistence of traditional landholding patterns and authority which derives from it. The government has followed a slow and gradualist approach in building centralized institutions to integrate local elites into a national economic and political process. As the authors point out, this may not be adequate in the long run, as reformers may seek dramatic action in the name of social justice. The stability afforded by traditional institutions may be destroyed before modern ones can mature.

Nepal, like other developing countries, confronts the frustrations of trying to achieve both political and economic modernization. Birendra seems to have heeded the advice of his Nepali and foreign advisors in emphasizing economic growth while placing political development in "cold storage." In other words, the king has been advised "to emulate the Shah of Iran" (p. 65).

Nepal's geographical position places it precariously because two giants, like "a root between two stones," a circumstance which governs its international posture. The chapter on foreign relations develops in some detail Nepal's recent international experience. Basically, Nepal can pursue isolation, attach itself to one of the great powers in the area, or try to follow a balanced policy. While the latter course has been followed since 1950, Nepal's welfare depends heavily on the good will of India. China, for its part, has chosen not to play a very active role in Nepal's affairs.

This welcome addition to the growing literature on Nepal is a measure of the considerable interest in that country. This interest is due in large part to the inherent social and physical beauty of the place, but even more so to the genuine hope that Nepal can grow and develop without the disruptions and disasters that other nations have experienced.

Louis D. Hayes

University of Montana

Politics and Class in Zaire: Bureaucracy, Business and Beer in Lisala. By Michael G. Schatzberg. (New York: Holmes and Meier, 1980. Pp. xi + 228. $29.50.)

Such is the fluidity of African stratification systems that it takes unusual analytic skills to hold them still long enough to delineate their contours. A major difficulty with class analysis is precisely its tendency to encapsulate social phenomena within a set of categories which in many instances turn out to be too rigid and artificial to convey properly the sense of flux in the African social landscape. Here is a book which explicitly confronts the issue, and does so constructively and intelligibly.

Unlike so many works in the Marxist tradition, Michael Schatzberg's work combines a rather undogmatic view of class phenomena with a keen sensitivity to the dynamics of social stratification. Along with Michael Cohen's work on the Ivory Coast (*Urban Policy and Political Conflict in Africa: A Study of the Ivory Coast,* Chicago, 1975) and John S. Saul's recent collection of essays on East Africa (*The State and Revolution in Eastern Africa,* Monthly Review, 1979), *Politics and Class in Zaire* is one of the most successful examples of the class-analysis approach to contemporary Africa. It is also a telling critique of Zairian politics under Mobutu. Through a judicious mixture of descriptive chapters and analytic sketches, the author conveys as much in the way of solid information about the Mobutist state as it does about the significance of class as a major structural component of this aberrant and ever-intriguing polity.

Much of Schatzberg's data is drawn from field work conducted in the town of Lisala, in the Equateur Region, and in a sense his book is first and foremost a fascinating case study of local politics in a small urban community. Almost every page bears evidence of his first-hand acquaintance with local actors, his privileged access to municipal and regional archives, and his own astuteness as a participant observer. The result is a remarkably detailed and vivid account of the mechanisms which enter into the process of class formation at the local and regional levels. His peculiar sensitivity to the significance of local resources is nowhere more tellingly revealed than in his effort to establish the relevance of beer—yes, beer—to the political economy of Lisala, an effort which has at least the merit of novelty. Who else would have thought of "the case of the disappearing depot" as a way of illustrating the decision-making process in Zarie? What better way of investigating issues of ethnicity than from the vantage point of bar ownership, wholesale houses and distributive networks? Running out of Primus in Lisala evidently means more than running out of beer. . . .

Schatzberg's book is not merely a richly documented case study of grass-roots politics; the Lisala arena provides him with a point of entry into the wider setting of Zairian politics. The contextual realities of Zairian politics are seen as the central variable in the background of class formation: historically, the rise of a "politico-commercial bourgeoisie" is traceable to a carry-over into the post-independence period of a social structure that had already begun to crystallize during the colonial days; the growing interpenetration of the commercial and political sectors after Mobutu's seizure of power in 1965 gave further cohesion to the symbiosis of wealth and power, with the party serving as the main avenue of upward social mobility. Wealth alone does not automatically confer power; access to power, however, opens up new opportunities for increasing—and losing—one's wealth. The overall picture conveyed is one of far greater fluidity than has generally been conceded by students of Zairian politics: "Social classes are constantly changing in response to differing socio-political contexts" (p. 31); furthermore, perceptions of class identity vary in such a way as to make it possible for an individual "to belong to differing class alliances at the same time" (p. 31). By explicitly emphasizing the dynamics of class formation by reference to historical and contemporary events, Schatzberg offers an interpretation of Zairian society which deviates markedly from conventional Marxist analyses.

Through a careful and eclectic handling of statistics and other types of evidence, including interview data, Schatzberg presents a richly nuanced view of Zairian society which goes far beyond the usual Marxist clichés about the relationship of African modes of production to class formation. The question remains, however, as to whether a similarly careful investigation of other local arenas might not reveal important discontinuities and deviations from the model proposed. One also wonders whether one can safely "evacuate," as it were, the ethnic dimension from one's analysis to the extent that Schatzberg does. Finally, while considerable attention is paid to the "linkages" between Lisala and the capital city, much less is said about how the surrounding rural sectors link up with Lisala. Even so, *Politics and Class in Zaire* is likely to remain a minor classic in the social science literature on Mobutu's Zaire, a field where classics of any kind are rare.

René Lemarchand

University of Florida

The Rancheros of Pisaflores: The History of a Peasant Bourgeoisie in Twentieth-Century Mexico. By Frans J. Schryer. (Toronto: University of Toronto Press, 1980. Pp. xii + 210. $20.00.)

This short book is stimulating and informative, though flawed in several respects. It is a local history of a county in the Mexican state of Hidalgo, which the author knows well, having lived there for a total of over a year's time during four separate periods in the late 1960s and early 1970s. Frans Schryer traces the county's byzantine politics of factional alignments and realignments, *caudillismo* and sudden death, since the Revolution of 1910, and arrives at several conclusions of general significance for the study of Mexico: to wit, that the better-off peasants, the small landowners he calls "rancheros," dominate rural politics; that this relatively well-to-do class is the major beneficiary of government programs, such as land reform, designed to help poor peasants; that ideology is used only for tactical purposes; that the Revolution and the development of coffee culture have reduced the pyramidal quality of the rural class system by increasing the number of landowners at the top, reducing the number of landless day laborers at the bottom, and fostering the growth of a class of small property-holders in the middle; that those less successful economically are more likely to be politically active; that when economic times are bad the number of political homicides rises.

Schryer has thus painted a convincing picture of a rural Mexico where politics is just another form of self-serving entrepreneurship, and where one gets ahead by exploiting others. The landless day laborers at the bottom of the scale are passive and apathetic, for the most part. The law is manipulated to serve individual and factional purposes and can often be bent into shape or overridden by the right political contacts—but not always, and it does have an independent existence and needs to be taken account of in one's calculations.

The local story provides an interesting counterpoint to the national history of the period. A new president's shift of policy provides a local faction with an opportunity of reviving some long-term objective; national prosperity changes the terms on which local struggles are fought; building a highway benefits one region at the expense of another. Political violence declines locally over time, as it does nationally; national political imperatives become more important as the country is increasingly centralized.

This makes a plausible, though not always completely convincing, picture. It is difficult for the reader to judge its accuracy because Schryer's conclusions are in effect incorporated into his narrative. His general depiction of the case feels realistic and politically sophisticated, but much must be taken on faith. One is a little uneasy at this, since there are clearly minor errors in descriptions of national politics and in the citation of sources, ambiguities in the presentation of at least one table, and various gaps in the data—such as the author's apparent ignorance of the full names of some of the characters in his story, including one whom he interviewed. Part of the difficulty here may be due to the fact that Schryer is an anthropologist writing history and political analysis. His subject matter transcends anthropology, but he retains the subjectivism which the anthropologist's normal research situation makes inevitable. Good book, could have been better.

Martin C. Needler

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Small Urban Centers in Rural Development in Africa. Edited by Aiden Southall. (Madison: University of Wisconsin African Studies Program, 1979. Pp. ii + 409. $8.25.)

This volume's roughly two dozen essays on small towns in Africa offer detailed vignettes on mechanisms and effects of change at a neglected level of analysis. Most of the essays are cross-disciplinary, touching in varying degrees on economic, social, cultural, and political structure and trends. Geographically, they span the continent, though Nigeria claims eight essays while only one deals with Southern Africa. The essays vary considerably in focus and quality, but collectively they offer valuable insights to at least three audiences: those interested in rural development, in spatial patterns of development and population redistribution policies, and in political change.

Small towns are often cited as potential catalysts or agents of rural development. Urban and regional planners and those concerned with patterns of migration often view revitalized and modernized small towns as important components in a broader strategy designed to divert migration from the largest cities and encourage less concentrated urbanization. Political analysts sometimes (probably too seldom) focus on small towns as the lowest level of the national administrative and political hierarchies, where rural people contact government agents and services, thereby shaping perceptions of government and—it is to be hoped—government responsiveness to rural needs.

This volume offers no simple generalizations on the role of small towns from any of these three perspectives. The main impression is of immense variation. Most of the towns described are in the 5,000-15,000 size range. Some trace their histories back for centuries, as pre-colonial centers of trade or administration. Others have mushroomed in the past few years, spontaneously responding to economic and political change or (in one pair of cases) deliberately created by government action. Some are thriving on new opportunities—the emergence of tourism on the coast of Kenya, or the establishing of a new university at Nsukka in Nigeria. Others stagnate (for example, a string of bus-stop hamlets off the Copperbelt in Rhodesia) or decline (a town across the lagoon from Lagos, formerly a trans-shipment point but now bypassed by improved transport links). The towns represent a range of economic functions—minor and major market centers, administrative seats, railroad nodes, centers of small industry (the tinsmiths of Awka, Nigeria) or in one case a large industry (Kima in Egypt). Their cultural and religious roles also differ markedly.

Perhaps most interesting to political scientists are the varying roles of these towns vis-à-vis both the broader national context and their own hinterlands. Americans may be particularly prone to idealize the potential political role of small towns as transmission points for rural preferences to reach higher state or national decision makers, and as centers where rural people can meaningfully participate in decisions regarding local development problems. Clearly, however, their capacity to play such roles depends on local history and social and political structure, and on the goals, organization and capacities of the larger political system. Just as small towns cannot promote rural development where national terms of trade for agriculture or international prices present powerful disincentives, so also they cannot represent rural interests nor act as centers for local participation if the systems of which they are parts do not seek or permit these functions. The cases suggest varied patterns: gross exploitation reflecting a profoundly corrupt national structure and partially perverse international links (Zaire); a case where local cleavages and competition, tied into a competitive national political context, produced substantial material benefits for the town and its locality (Ronald Cohen on Biu in Nigeria); another where town notables act as interpreters and buffers between national agents and local rural leaders, while being kept at a bit of a distance by the latter (Nicholas Hopkins on Kita in Mali).

In preparation for the research and conference leading up to this volume, hypotheses were formulated (reproduced in an appendix) attempting to identify mechanisms and conditions under which small towns could effectively promote rural development. The studies themselves, however, prompt Aiden Southall to suggest in his introduction that "Most small towns appear as the lowest rung of systems for the oppression and exploitation of rural peoples" (p. 2). Trends toward centralization and international penetration exacerbate this tendency. Yet it seems to me that many of the cases—including some which the authors present as examples of exploitation—demonstrate instead the mixed benefits and costs of change, and the immense range of local, as well as national and international factors bearing on who gains and who loses. Ronald Cohen's essay strikes a more balanced position, as does Crawford Young's on the changing role of African states and the extent to which dependency theory is useful in understanding the changes. To move beyond sweeping and probably misleading generalities, we need to focus more precisely on mechanisms and conditions under which small towns play various roles. These cases are most valuable for their (mainly implicit) suggestions on such mechanisms and conditions.

JOAN M. NELSON

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Poverty in the United Kingdom: A Survey of Household Resources and Standards of Living. By Peter Townsend. (Berkeley: University of California Press, 1979. Pp. 1216. $37.50, cloth; $15.95, paper.)

Poverty appears on America's agenda when we have depressions, urban riots, or an interested president, but currently it is out of fashion. In Britain, however, the state of the poor is a continuing concern for which each successive government is held answerable. This steadier focus can be credited in large measure to the well-publicized studies of Peter Townsend and the other diligent researchers associated with Richard Titmuss, a professor of social administration at the London School of Economics from 1951 until his death in 1973. In 1952, Peter Townsend attacked the prevailing assumption that the "welfare state" created in 1945-48 had eliminated poverty. Questioning the subsistence poverty standard on which optimism about postwar social progress was based, Townsend proposed a relativist concept as the more realistic measure. *Poverty in the United Kingdom* is the fulfillment of Townsend's work since 1952 in operationalizing that idea.

This mammoth volume reports on the most well thought-out, wide-ranging poverty survey ever done in Britain, which is probably to say anywhere. In 1968-69, a specially trained team administered a 40-page questionnaire to a national sample of 3,260 households, comprising over 10,000 individuals. But the book provides more than a poverty count in one nation. Townsend follows the ameliorist, fact-gathering tradition of Charles Booth, the Webbs, Seebohm Rowntree, and William Beveridge, while extending it; unlike his predecessors, he does not eschew grand social theory. The book's originality lies in its effort to use an empirical survey of the distribution of resources in British society in order to outline a general explanation of the structure of inequality in capitalist societies.

In theory, U.S. and U.K. welfare benefit levels are based on the cost of food and other necessities deemed requisite for physical efficiency and social functioning. In practice, governments adjust those levels in response to prices, wages, and changed opinion. Thus, what is actually a relative, politically determined standard is widely taken as an absolute, scientific standard. But Townsend rejects the simple option of updating such poverty lines and clarifying their ambiguous application. The underlying subsistence rationale is wrong. People are poor, Townsend argues, when they are excluded from the diets, household amenities, educational opportunities, work conditions, and physical environments which are "customary, or are at least widely encouraged or approved, in the societies to which they belong" (p. 31). Poverty could increase as a society got richer.

The survey tries to demonstrate how such a poverty standard derived from conventions can still be established independently of subjective attitudes felt by the poor or toward the poor. The distribution of a wide range of resources enjoyed by the general population was determined, and a rough index of a national "style of living" identified. Income strata were then ranked on each and all resource dimensions according to a "deprivation index." Townsend hypothesizes that when income is severely lacking, participation in society's normal activities declines precipitously. This dropping-off point should serve as the poverty line.

Not surprisingly, the survey found considerably more in poverty—12½ of the U.K.'s 55½ million—when using the deprivation standard than when the government's poverty line was applied. There was evidence in some household categories of an income threshold below which social isolation increased rapidly, although the data were judged inconclusive. Even using the state's own standard, however, Townsend found more poor people than other studies of that period would have predicted.

Of great interest is Townsend's modified class explanation of poverty. Separate chapters describe each of the demographic categories constituting most of the relatively deprived: unemployed, low-paid, and older workers; the disabled; handicapped children; one-parent families; the elderly. Its concentration within these "social minorities" leads Townsend to reject individualistic determinants of poverty, but the poor's dispersion across different ethnic groups and communities makes subculture and area theories inadequate also. Townsend rejects radical and dual market theories, moreover, because they focus primarily on the effects of the labor market and wage system, and neglect the nonworking population.

It is not simply relations to the productive process that determine the chances of poverty, Townsend suggests, but also the influences of hierarchical private organizations, state bureaucracies, professions, associations, and clientele interests in helping "to reproduce but also to modify" (p. 89) the class structure. A complete explanation of modern deprivation must include all institutions that set the standards for the style of living widely expected, and then govern access to the work roles and resources required to meet these standards. By creating standards and allocating privileges and status, these institutions and their rules differentiate the population, consigning some to roles of dependency. Policy constitutes society.

The many different topics and methods found throughout the book's 27 chapters and 13 appendices hold lots more for various sociological, methodological, and policy specialties. Two chapters on the problems in Britain's centralized public assistance system and its local means-tested services, for example, could be read profitably by reformers looking to the federalization of all American welfare. All readers will be moved by the chapter of case narrative revealing the impacts of poverty on specific families.

Townsend and Britain's poverty action groups have influenced governments not because there has been wide acceptance of the idea of relative poverty or its underlying causal theory. They have had influence because they have documented unmet needs under existing welfare programs, using the government's own poverty criteria. The Titmuss group has largely employed the traditional Fabian strategy of publicizing their studies in the London press and engaging in top-level dialogue with government ministers. This approach is sensible, given the obvious difficulties of directly mobilizing statistical categories such as "households with borderline disabled under 65." Any ambitions to mobilize wide political support behind an anti-deprivation theme would also have to confront another brutal fact. As the survey itself revealed, most people defined poverty not in the relative terms Townsend does, but in the way the government does, where the figures are less staggering.

MICHAEL S. LUND

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The Soviet Union: Looking to the 1980s. Edited by Robert Wesson. (Millwood, N.Y.: KTO Press, 1980. Pp. ix + 288. $25.00.)

This volume, with 16 contributors, takes a look at Soviet domestic and foreign policy with an eye on the future. Most of the authors are well known and highly respected, which on the surface augurs well for an up-to-date, provocative treatment of Soviet politics. The quality of the contributions is disappointing, however. Some chapters are excellent; others are only fair. The more distinguished chapters are those written by Jan Triska ("Soviet-East European Relations"), Joseph Berliner ("Economic Prospects"), Gertrude Schroeder ("Prospects for the Consumer"), Teresa Rakowska-Harmstone ("The Nationalities Question"), and Frederick Barghoorn ("Political Dissent").

Although the book deals with foreign as well as domestic affairs, it is the question of domestic change that ties the contributions, or at least most of them, together. In the words of the editor, Robert Wesson, is the Soviet Union "approaching major change"? (p. vii). The answer that Wesson gives for his contributors is affirmative: "It is hence bold to entertain the thought that there may now be severer strains in prospect, possibly entailing instability of the long stable system. *We* [italics added] are tempted, however, to do so, viewing the visible trends of recent years" (p. 264). This conclusion varies, in fact, from the assessment made by most of the contributors. Four pay little if any attention to the central question, and eight conclude that major change is not in the offing. Only four contributors—Berliner, Wesson, Rakowska-Harmstone, and Andrei Amalrik—see significant change on the horizon, and even here there is a difference. Berliner suggests that the post-succession leadership may seriously entertain a within-system organizational reform, a modern version of the New Economic Policy (pp. 105, 198), whereas his three colleagues predict system change, with Amalrik going so far as to depict the replacement of Soviet "totalitarianism" (p. 258) with "Russian fascism" (p. 255).

The volume offers little that is new. It does, however, assist the reader in recalling the highlights of recent Soviet actions, both domestic and foreign, and it contains projections into the 1980s that for the most part are well argued. The book is a "must" for large libraries, but the return on a capital investment of $25 would for most individuals be marginal.

JOHN H. HODGSON

Syracuse University

Political Culture and Soviet Politics. By Stephen White. (New York: St. Martin's Press, 1980. Pp. xi + 234. $22.50.)

Stephen White seeks to provide a comprehensive view of the evolution of Soviet political culture. Despite the inherent difficulties and some shortcomings in the use of available evidence, the result is a significant contribution to the Anglo-American study of the Soviet system. The major strengths of the book are the clarity of the presentation, the effective effort to relate the Russian experience to the contemporary Soviet situation, the thorough use of other relevant work, and the relating of the political culture theme to the heterogeneity of the Soviet population.

White demonstrates a superior ability to organize a complex body of findings and to minimize dependence on jargon. Such efforts result in a rather concise volume that, nevertheless, covers much ground.

The historical dimension takes up approximately one-third of the book. Although this section is the least valuable to the specialist, the major sociological and political aspects of the tsarist heritage are effectively linked to current Soviet patterns.

The diverse sources used include memoirs, sociological studies published in the Soviet Union, and interviews of Soviet emigres during various eras. The heart of the book is a direct comparison of the findings of Alex Inkeles and Raymond Bauer (*The Soviet Citizen*, Harvard, 1959) and recent surveys of Soviet Jews and other emigres. The results of a burgeoning Soviet survey research literature are also stressed.

Several significant shortcomings relating to the use of evidence need be noted. First, White tends to use surveys of Soviet Jews who had emigrated to the United States and Israel as if this group represented a cross section of the Soviet population rather than a distinctive subgroup. Such studies, including several of White's own, are consistently identified as studies conducted "with Soviet emigres in Israel and the USA" (see p. 96). The term "Jew" does not appear in the book's index and is rarely used in the text. It is doubtful that other researchers would accept his universalization of the Soviet Jewish experience.

Another problem pertains to the use of sociopolitical surveys developed by official Soviet agencies. White makes some efforts to distinguish between credible and dubious published findings. Yet the reader is left wondering about the impacts of varying procedures used by the Soviet researchers as well as presumed official decisions to alter or withhold unwelcome findings. For example, it is noted that not all official surveys are developed with anonymous respondents.

Not surprisingly, White's findings seem strongest when he stays closest to his best sources of data and the specific elements of political culture. An example of this is his conclusion, "There is no evidence of a movement towards more participation and consultation norms, in these respects at least, over a period of major and far-reaching social and economic change" (p. 175).

Yet his broader generalizations do not always seem to be based on sufficient evidence. Mass support for the Soviet "system" is said to coexist with limited commitments to the regime and to Marxist-Leninist values. This conclusion may well be essentially true. Yet additional unassailable evidence is needed to sell this centrist perspective to skeptical readers.

RICHARD SIEGEL

University of Nevada, Reno

International Politics

Communism in Africa. Edited by David E. Albright. (Bloomington: Indiana University Press, 1980. Pp. viii + 277. $12.95.)

This first-rate compendium consists of eight essays, three of which are revised, enlarged, and updated versions of articles published originally in *Problems of Communism*. All contributors are experts who provide valuable information and insights concerning the impact of communism on Africa.

To ensure a degree of coherence, David Albright uses the dynamics of Soviet-African relations as an organizing framework. Such a focus emphasizes the role of the USSR, but it also leaves room for other communist powers' involvement and allows exploration of the roles that noncommunist elements have played in communist successes and failures. Undoubtedly, the Soviet superpower can and does interact with other communist and non-communist states with unmatched intensity. The thesis of the book is that the primary significance of the non-Soviet activities lies in their impact on the enterprises of the USSR in Africa.

In keeping with the focus of the book, each chapter addresses itself to a particular issue bearing upon the dynamics of Soviet-African relations. Colin Legum's assessment of changes over time in the attitudes of African governments and movements toward Moscow seriously underestimates the positions of power and influence the Soviets have gained in the last few years. He fails to take into account the fact that Soviet-African relations have always been determined by Soviet decisions based on the degree of Soviet interests involved. This view is expressed clearly in Albright's study where he concludes that Soviet policy toward Africa depends on "the relative priority that (Moscow) attaches to the continent . . . and the opportunities that Africa presents" (p. 64). T. Wilson's essay emphasizes the effects of the long-standing Russian associations with Africa on the Soviets' recent policies toward the continent. Jiri Valenta's examination of Soviet decision making is based on the intervention in Angola, and supports the view that events, perceived as opportunities, have shaped Soviet behavior more than their behavior has shaped events. Marina Ottaway focuses on the interaction of the Ethiopian and Mozambican regimes with Moscow and the relationship between Marxist-Leninist theory and practice in those countries. Edward Gonzales' study of Cuba's role in Africa on Soviet Africa policy points to the most serious weakness of the volume. There is no discussion of the important role of the German Democratic Republic in Moscow's Africa policy. To ignore Cuba's German communist counterpart in the Soviet penetration of Africa is a serious mistake, especially in view of the overwhelming evidence that has been discovered.

George T. Yu's contribution deals with aspects of Sino-Soviet rivalry in Africa and concludes that Chinese communist influence on Soviet behavior will probably "remain quite restricted in the foreseeable future" (p. 188). W. S. Thompson explores "The African-American Nexus in Soviet Strategy," and draws attention to Soviet concern with the minerals in Africa and the sea lanes around the continent. By being able to cut off shipments of crucial minerals to the West and endangering the flow of oil from the Middle East, the Soviets have from a strategic point of view outflanked the Middle East, which is critical to Western survival. In the last chapter the editor discusses the prevailing instability in Africa and concludes that "the atmosphere on the continent will in all probability be favorable to increased Communist involvement" (p. 233). This, then, is the general impression created by this timely and important contribution to the literature on Africa's role in international relations. Soviet involvement can be expected to continue, and it will probably grow in the future. The implications for American foreign policy in the 1980s are unmistakable.

ROBERT M. BIGLER

University of Nevada, Las Vegas

Conflict and Intervention in the Third World. Edited by Mohammed Ayoob. (New York: St. Martin's Press, 1980. Pp. xi + 261. $27.50.)

The Third World has been the principal arena of violent conflict, and the only scene of interstate military engagements, during the past several decades. Largely as a product of the Third World's colonial heritage but partly as a result of contradictions whose origins are independent of the legacy of external dependence, conflicts of interest have outstripped the social and institutional bases for their peaceful resolution. Moreover, it is very likely that the human and material costs of these antagonisms will continue to be heavy in the foreseeable future. Therefore, this book addresses an important topic; whether it does so entirely successfully is debatable.

More often than not, the edited volume contains a collection of essays of uneven quality and of far-flung perspectives. This is not so here and the uniformity of style and of discourse (though not necessarily of political opinion) is truly unusual—no doubt explained by the fact that, of the eight authors, five are associated with the Australian National University (and only one is affiliated with a non-Australian institution). The first and last essays (by T. B. Millar and Mahommed Ayoob, respectively) are predominantly theoretical, while the remaining seven are case studies of the principal modern instances of Third World conflicts, from Cyprus to Indo-China. On the whole, the theoretical pieces are rather loosely speculative and the other contributions, while factually comprehensive, are analytically modest. The main purpose of a case study, of course, is to present an interesting constellation of facts relevant to a specific social entity: but, with fully *seven* such essays, it should have been possible to draw analytically solid and empirically well-grounded generalizations. This, in my opinion, has not been done. The essay which carries the final burden of attempting to achieve this is the editor's concluding piece. What we are presented with is a broad-stroke indictment of the major powers as cynical fomenters of Third-World antagonisms, but the attempt to trace the various causal mechanisms which are involved is startlingly skimpy and gives virtually no recognition to possible rival hypotheses (some of which are suggested by the case studies themselves). It is not that Northern responsibility can be plausibly denied, it is merely that its nature is not nearly as simple as Ayoob seems to feel; in short, the valuable empirical material mustered in the case studies has not been done adequate theoretical justice.

Perhaps the most interesting essay is D. J. Goldworthy's piece on South Africa, in which the author convincingly traces the contradictions of a system which has achieved substantial economic growth by the sort of economic repression which deprives it of the internal talent and markets needed to perpetuate this growth. The international implications of this contradiction are particularly well described.

In conclusion, the book contains a wealth of relevant factual information on some of the politically most volatile regions of the world; it is interesting but not, however, as useful as it could have been were it an empirically more rigorous and conceptually more sophisticated volume.

MIROSLAV NINCIC

University of Michigan

Office Without Power: Secretary-General Sir Eric Drummond 1919-1933. By James Barros. (New York: Oxford University Press, 1979. Pp. xii + 423. $41.00.)

James Barros' *Office Without Power* is the first book to appear on Sir Eric Drummond's tenure as the first secretary-general of the League of Nations. Those who write textbooks on international organization frequently use Drummond as a prototype of the discreet, behind-the-scenes administrator and facilitator of international agreements who eschews public exposure and publicity either for his own positions on issues or for the ideals of the organization's charter.

Drummond adopted this role because he was aware that the great powers in his day would not and could not rely upon the newly created League to solve major international issues. Thus, he sought to be perceived as a neutral international officer who would merit the confidence of *every* League member and who would always be ready to help in international conflicts to whatever extent possible within the principles of the Covenant (p. 263). While the League could never dictate the settlement of disputes, especially those involving major powers, Drummond thought that the organization could provide "face shaving machinery" in conflicts involving prestige and serve as "an enormous safety valve" for the volatile boundary and minority disputes that plagued the first decade of the interwar period (p. 41). Any more grandiose role for the organization would come slowly, he thought.

The great contribution of Barros' book is his absolutely thorough and detailed description of Drummond's behavior within this role conception. Drummond's quiet activity is revealed in detail for every major conflict during the first 13 years of the League's history. Barros' study also makes it clear that while Drummond worked quietly and discreetly he was in no way nonpolitical. He unceasingly gave advice and made suggestions on both tactical and substantive policy issues to great and small powers alike.

Even in his own terms, however, Drummond was not completely successful. He never attained the confidence among such revisionist powers as Germany and Italy that he was able to gain from Britain, the Scandinavian states and the members of the Little Entente. Also, because he had decided to work with the Great Powers, especially Britain, Drummond's efforts were always heavily constrained by the policies of these states. And, of course, there were cases where persuasion simply failed, in Corfu and Manchuria most notably. Drummond, however, was never under any illusions about the limits of his own efforts or those of the League. His tenure was one of many frustrations and disappointments, all of which he felt deeply.

The organization of *Office Without Power* is largely chronological and geographical. After discussing the development of the office of the secretary-general, Drummond's conceptions of international politics and international organization, and his behavior in 1919 and 1920, Barros describes Drummond's activity in separate chapters on the German question, Latin America, Italy and Manchuria. Because of this largely chronological framework, others will have to reorganize Barros' material to answer more analytical questions. For example, if one wants to know what kinds of influence attempts Drummond made to what kinds of powers in what kinds of disputes and how successful these attempts were, one will have to rework Barros' data. But, happily, the data is now there—data that was unavailable or inaccessible before. And for this we have Barros to thank.

Clearly, *Office Without Power* will be the basic reference work on Eric Drummond's tenure in office for a long time to come.

STANLEY J. MICHALAK, JR.

Franklin and Marshall College

Terrorism and Global Security: The Nuclear Threat. By Louis René Beres. (Boulder, Colo.: Westview Press, 1980. Pp. xii + 161. $18.50.)

Louis René Beres is at his best when he describes contemporary political terrorist movements. He correctly states that such groups are engaged in total war against prevailing social, political and economic values and structures. Terrorists are thoroughly committed to the righteousness of their cause and stand ready to sacrifice their lives and the lives of others for the movement.

Nuclear terrorists are different because their actions may, when compared to those of ordinary terrorists, potentially cause a greater degree of damage and suffering. They can steal nuclear weapons, fabricate their own nuclear explosives, or sabotage nuclear power reactors. The nuclear genie has been released, there are no more secrets. Terrorists could intimidate governments by threats, wreak havoc if their demands are not met, and provoke regional and global nuclear wars. In addition, Beres correctly points out the need to restrict the proliferation of nuclear weapons. Clearly, the non-proliferation treaty and fuel controls are not adequate. However, he offers no concrete method of preventing the spread of nuclear explosives.

Beres should have limited his focus to an analysis of terrorism and its nuclear implications. Instead, he offers some extraordinarily superficial views on military strategy and world politics, and then suggests that the world may soon end if the present system of relationships among states is not altered.

Like many of the nuclear pacificsts of the 1960s, Beres' vision of the future is cataclysmic. Nuclear weapons and terrorists, however, are not the root causes of disaster. Realpolitik and the politics of national defense and interest are the true culprits. States must discard such narrow concerns and become conscious of their "planetary identity." There is, according to the author, a desperate need for a new world order to promote cooperation rather than competition. The current system, he believes, cannot last out this century. Beres provides several probable models for a new order, none of which is adequately explained nor explored. Nor, and this is most significant, does he indicate how the possibility of nuclear terrorism will be diminished in a system wherein national interests are redefined and "planetization" is promoted. He assumes, without adequate justification, that a new world order will produce circumstances wherein terrorists will become less dangerous. Beres does not convincingly demonstrate why realism is so awful nor does he give the reader any reason to believe that "planetization" is a viable alternative to the current configuration of world politics.

Unfortunately, the scope of this work does not live up to the promise of the title. Beres' superficial attempt to analyze and reform international politics from the perspectives of nuclear terrorism will impress few academicians and probably no political leaders.

ANDREW C. TUTTLE

University of Nevada, Las Vegas

Deviance, Terrorism and War: The Process of Solving Unsolved Social and Political Problems. By John Burton. (New York: St. Martin's Press, 1979. Pp. xv + 240. $22.50.)

This is a wise and convention-shaking book, nudging us toward a less coercive political science. The thesis is that a shift is taking place in world politics from a "coercive paradigm" to a "needs paradigm." The shift is embodied in a participatory process of "problem solving" that replaces customary forms of "puzzle solving." The motivation for the shift is failure of coercive institutions to satisfy human needs. The task of political science is to interpret the shift for the benefit of all involved so that the transition can be quicker and less destructive. Political science, however, does not prescribe solutions to problems; it assists processes by which parties in conflict identify and accept them.

The "navigation point" to guide political action and analysis is the concept of "universal human needs" that Burton, after Paul Sites in *Control, the Basis of Social Order* (Kennikat, 1973), defines to include needs for consistency in response, stimulation, security, recognition, distributive justice, rationality, meaning, and control. Burton adds the need for "role defence" (protection of acquired need satisfactions) and also stresses need for "identity." Criminals, terrorists, and war leaders are all assumed to share the same needs as their victims.

This line of thought leads Burton to assert a view for which this book is most likely to be remembered. Others have said it in different ways, but perhaps no political scientist has stated so boldly the thesis that when violence occurs—including crime, terrorism, and war—the perpetrators are not to be regarded as "deviant." Rather their behavior is to be taken as evidence that the dominant institutions and structures of a local community, nation, or international polity are "deviant" to the extent that they have failed to satisfy needs of persons affected by them. Therefore the grand task of political science is to help parties in conflict understand this deviance and to work together toward common need satisfactions. Burton rejects the conventional zero-sum objection to this approach and advocates instead positive-sum outcomes in which all gain something.

Constant return to human needs as criteria for analysis and action enables Burton to question assumptions that have led to theoretical, disciplinary, and administrative compartmentalization. For example, Burton questions the reasonableness of the assumption that the problem of communal conflict in Northern Ireland ought to be solved by the British Home Office while that in Cyprus ought to be solved by the Foreign Office.

Overall, Burton's book suggests an incipient paradigm for a political science of human needs. One of the advantages of such a general framework is that it assists identification of opportunities for internal development as well as for external assistance.

Five are apparent. First, the emerging paradigm requires basic and applied theoretical development. Although Burton does not consider the problem of combining consciousness-raising hermeneutics and positivist social science with the clarity of Robert L. Heilbroner's later discussion in *Marxism: For and Against* (Norton, 1980), he appreciates the need for testable and falsifiable propositions. They are still needed, offering a challenge to basic and applied theoreticians. Second, further development of the paradigm should take account of the unmentioned Lasswell-McDougal framework of policy science—and vice versa. This is not to prejudge superiority but to recognize fertile affinity. Third, although Burton, like Lasswell, does not center attention upon the concept of "leadership" in either practical politics or in applied social science, his work is rich with leadership-related ideas scattered in discussions of "power" and "authority" as well as in inadequately indexed references to "leadership" itself (e.g., references to third party intervention as "leadership behaviour," p. 219). More explicit recent political science interest in the concept of leadership can easily be brought to bear, including needed rediscovery of Martin Tarcher's neglected but potentially seminal view of leadership as applied social science in *Leadership and the Power of Ideas* (Harper, 1966). Fourth, Burton's clear commitment to lessened violence needs to be advanced to embrace the social science literature on nonviolent behavior that is beginning to appear, pioneered especially by sociologists such as Gene Sharp, George Lakey, and Severyn T. Bruyn. Fifth, the moral commitments of Burton's human needs scientist and Lasswell's policy scientist both need to be made much more explicit as a scientific problem. It is a myth that we can help others solve life and death problems without controlled awareness of how our own values and needs, including our silences, affect the process.

By questioning assumptions and suggesting new ways out of old difficulties, implying that political science itself may be "deviant" in its capacity to serve universal human needs, Burton's book helps to envision new steps forward in disciplinary advance. It deserves a wide reading among political and other social scientists.

GLENN D. PAIGE

University of Hawaii

The Politics of Grandeur: Ideological Aspects of de Gaulle's Foreign Policy. By Philip G. Cerny. (New York: Cambridge University Press, 1980. Pp. xi + 319. $29.50.)

Why did Charles de Gaulle strive in such a dramatic manner "to upset the flower pot" of international relations during his years as president of the Fifth French Republic? This question, which has never ceased to fascinate students of French and international politics, is the central concern of *The Politics of Grandeur.* Philip Cerny answers the question in an original fashion, not through the introduction of new data but rather through the development of "a new synthesis, based upon a new interpretation of the significance of Gaullist policy" (pp. 2-3).

The first part of Cerny's book focuses on the "ideological roots and purposes of de Gaulle's foreign policy." Four chapters of part 1 deal with the personal, philosophical and structural bases of Gaullist policy; the fifth chapter attempts "to chart a course through the complex waters of generalization" (p. 98), analyzing de Gaulle's policy as an example of efforts to achieve national integration or authority legitimation through foreign policy leadership. The second part examines de Gaulle's "grand design and foreign policy praxis." After briefly describing the structure and development of the grand design, Cerny devotes three chapters to a study of Gaullist treatment of "the Atlantic question," and then concludes with an assessment of the impact of de Gaulle's foreign policy on domestic politics in France.

Cerny's central thesis is that "the purpose of de Gaulle's foreign policy was not the attainment of glory and power for France for its own sake," but rather the creation of "a new and more profound sense of national consciousness" which would transcend traditional French political divisions and thus allow for the maintenance of domestic stability and the legitimation of the Fifth Republic's institutions (p. 4). As Cerny acknowledges, he is by no means the first analyst to contend that "the Gaullist notion of *grandeur* was a significant symbolic force for the creation of domestic stability and a sense of national identity" (p. 6). However, his essay is the first attempt to develop at length the argument that *grandeur* was pursued *primarily* for such domestic purposes. De Gaulle's "international revisionism was a function of his aspirations for the cultural and social integration and development of the French people by means of a common purpose, and not the other way around" (p. 6). Within the analytical framework presented by Cerny, "the acid test" of Gaullist foreign policy "was domestic impact" (p. 90). Viewed from this perspective, de Gaulle's ambitious foreign policy initiatives deserve much higher marks than they are usually accorded. Cerny concludes that de Gaulle's "symbolic strategy" (p. 259) was essentially a success, fulfilling "a vital function of normative integration" (p. 268) by building a consensus not merely on foreign policy but also "quite clearly on the acceptability of the political system as a whole" (p. 263).

As compelling as Cerny's account may seem, it must be said to suffer from two major problems. First, his portrayal of de Gaulle simply cannot be substantiated on the basis of the available evidence. Cerny asks that we "read between the lines of Gaullist rhetoric" (p. 272), but he provides no sufficient reason to ignore the lines which indicate that *grandeur* was valued at least as much for international as for domestic purposes. Second, his images of a de Gaulle chiefly concerned with domestic stability and of a French public successfully integrated into the new system of authority are both difficult to square with the events of May 1968. Was not the May revolt caused at least in part by what Stanley Hoffmann has termed the "subordination of domestic politics to the primacy of international affairs and the requirements of status"? (p. 89). Cerny implicitly rejects this notion, but he provides no alternative explanation and indeed virtually ignores the subject of May 1968 and its aftermath.

Despite its flaws, Cerny's book is definitely a worthwhile contribution to the literature on French foreign policy. Perhaps its greatest virtue is that it shows clearly how and why the "*gaullien* style of politics" in international affairs continues to be "one of the most compelling options of a President of the Republic" (p. 269). Readers of *The Politics of Grandeur* will understand how Giscard d'Estaing's test of the neutron bomb represents not only the progress of French technology, but also the persistent domestic appeal of the foreign policy launched by de Gaulle.

JOHN T. S. KEELER

University of Washington

The New International Economic Order: A U.S. Response. Edited by David B. H. Denoon. (New York: New York University Press, 1979. Pp. xvi + 346. $20.00.)

In 1976, concerned by the turmoil created in the wake of increased oil prices and challenges from advocates of a New International Economic Order, the United Nations Association of the United States (UNA-USA) gathered together a group of distinguished Americans for the avowed purpose of evaluating claims made by the LDCs and to make policy recommendations on particularly promising responses. This book is the technical report of the scholars commissioned to carry out these studies. The book is divided into three parts (trade, commodity policy, and capital flows) which correspond to the three panels set up by the UNA-USA Economic Policy Council. The Economic Policy Council, listed in an appendix, reads like a "Who's Who" of representatives of U.S. interest groups. And therein lies a major problem with the book.

The Economic Policy Council decided that the project would have two major foci: proposals for restructuring the international economic system which yield *joint* gains for developed countries and LDCs and a package of recommendations which would respond to these challenges but yet be sufficiently *pragmatic* to be of use to the U.S. Congress and executive branch. Since Congress and the executive branch have been notoriously hesitant to confront many of these ticklish issues, the project faced severe limitations from the beginning. The result, written almost exclusively by economists, is heavy on models and light on worthwhile suggestions. The political aspects of a very important set of international political issues are almost entirely ignored.

The book is a collection of technical essays and it is difficult to characterize the result as anything other than mixed. It wanders over a great deal of territory and hits upon a number of topics, but gives no good reasons for the selection of some and elimination of others. And, aside from a very fine introduction by the editor, there is very little that integrates the essays.

The best section of the effort deals with commodity policy. Willard Cochrane does an excellent and complete job of assessing the international grain market and analyzes proposals for alleviating market fluctuations and potential future shortages through creation of a world grain reserve. Among the ideas found in his four pages of excellent policy suggestions is a scheme that would commit importers and exporters of grains to hold grain reserves of at least 70 million tons; thus Cochrane walks a very delicate line between those who advocate no organized reserves and those who argue for an international organization to do the same thing.

The chapters by Smith, who deals with the tin market, and by Woods, who concentrates on the world aluminum/bauxite market, are likewise complete and are highly recommended reading for those interested in international commodity markets. Smith argues that tin is of little economic significance to the United States; the importance of U.S. membership in the Fifth Tin Agreement is more symbolic and less economic than might be supposed. He urges that the U.S. play a constructive role in the organization since it is a pretty much cost-free method of demonstrating willingness to collaborate with exporters of commodities. The selection by Woods is more notable for the completeness of the analysis than any suggestions that he makes; it is essential reading for anyone interested in the aluminum market.

The rest of the chapters in the book are somewhat disappointing because, in general, they say nothing very original and have become outdated because of better work more recently published.

The section on trade is unexciting and the relationship of large sections of these chapters to demands of advocates of a New International Economic Order is never made clear. Thus the authors tell us that computer models indicate that multilateral tariff reductions reduce prices in all countries and that the overall effects on employment are quite modest. But the effects vary among industries. In the United States, for example, multilateral tariff reductions hurt iron and steel, textiles and apparel, while footwear, electrical goods and transport would be helped by such a move. This, of course, creates political problems which extend beyond the scope of the book. The section on capital flows concentrates on the debt problems of less developed countries in light of the first massive increase in petroleum prices (the impact of the 1979 increases is not covered). The authors attempt to remain analytical, and consequently say very little that is new or original about the debt problem. Little matter, since this section has been superseded by several more complete books on the subject.

In summary, this uneven book unwittingly illustrates the general paralysis of United States policy in response to demands for a New International Economic Order. The council's mandate seems to have been to look at these issues but not to suggest anything that would offend anyone. And as is true of U.S. policy, the result is timidity. There is very little that is new or creative either in analyses or policy suggestions, and again, like U.S. policy, the result is intellectual bankruptcy. As a U.S. response to demands for a New International Economic Order, the book is lacking. If a vote for creativity were taken at this point, the NIEO would certainly win round one.

DENNIS C. PIRAGES

University of Maryland

The Middle Eastern States and the Law of the Sea. By Ali A. El-Hakim. (Syracuse, N.Y.: Syracuse University Press, 1980. Pp. xv + 293. $27.00.)

The Middle East has long been a turbulent region, seemingly lurching from one crisis to another and demanding our almost constant attention. The region has not, however, produced comparable drama in marine affairs except for sporadic threatened and actual interruptions of navigation. Nevertheless, the importance of its petroleum, waterways, fisheries and votes at the Third United Nations Conference on the Law of the Sea (UNCLOS III) make necessary a detailed and authoritative analysis of its role in the development of the Law of the Sea. This book is an important contribution toward such an analysis.

The book is organized into two parts. The first covers general practice and policies with sections devoted to existing claims to offshore jurisdiction and to trends as reflected at UNCLOS III, including discussions of positions taken by states of the area at the conference and of recommendations of the Arab League. Part 2 covers Specific Regional Issues: the "legal problems of offshore boundaries in the Arabian Gulf," "legal status of the Gulf of Aqaba and the Straits of Tiran," and "legal aspects of the Red Sea hot brines and other metalliferous mud deposits." The first two of these topics get very detailed treatment, the third much less, but still more than can readily be found elsewhere. The final paragraph of the four-page general conclusions is worth quoting (p. 192):

> In conclusion, while there is much to be said for regional cooperation and agreements, it is to be hoped that a universally acceptable system of cooperation will supersede those regional arrangements. It can be seen from the foregoing chapters that, in so far as a coherent Middle Eastern point of view emerges, there is great potential for reconciling it with that of the wider international community.

This commendable universalist view is contradicted, however, by the author's parochial view of anything regarding Israel.

The book contains seven appendices, mostly texts of relevant national legislation; a helpful list of abbreviations; extensive and most useful notes; a selected but still impressive bibliography and a good index. There are 17 simple maps, many of which lack legends to explain the symbols used. The writing is generally smooth and careful, a good blend of law, economics, politics and geography, with few typographical errors and a minimum of jargon. It does have a serious weakness, however.

El-Hakim does not include Israel among "the Middle Eastern States" and scarcely mentions Israel except in connection with the Gulf of Aqaba and the Straits of Tiran, to which he devotes nearly 20 percent of his text, even though Israel has played a significant role at UNCLOS III and is a major maritime power in the region. His agreement with the Arab position on the Aqaba-Tiran issue is understandable, but not his use of the name "Palestine" where "Israel" should be on two maps. On one of them (p. 134) he even shows a number of tiny settlements in Sinai and Saudi Arabia but only a blank space where the Israeli port city of Eilat should be!

In his introduction, El-Hakim stresses that his book is not comprehensive, so omission of certain topics is not necessarily a fault. While the book is not nearly as complete as the loose-leaf series published by Oceana on Latin America, Eastern Europe, Western Europe and North America, and East Asia and the Pacific Islands and the Law of the Sea, it does nevertheless partially fill a gap left by this series and, despite the weakness noted above, is a most welcome addition to the literature.

MARTIN IRA GLASSNER

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America and the Third World: Revolution and Intervention. By John L. S. Girling. (Boston: Routledge & Kegan Paul, 1980. Pp. xii + 276. $25.00.)

John L. S. Girling of the Australian National University, earlier a member of the British Foreign Office research staff, has written a difficult but important and engrossing book. It is of a genre of which we need more: thorough, painstaking and penetrating analysis—not the mere recounting or judging of the purposes, means and results of U.S. foreign policies. At the same time, Girling's own views and conclusions illuminate the book.

The first 100 pages of this succinct work coolly scrutinize the motivations and structural contexts of Third-World revolutionary movements, emphasizing throughout the four "pivotal" strata Girling identifies in "underdeveloped" social structures: "peasant dependence, radical rootlessness, military caste loyalties and either bourgeois absorption in or management of the system" (p. 100). Girling finds neither the pluralistic, patron-client or neo-Marxist (dependency) models satisfactory as "global" theories of revolution, although the last of these, without being necessarily predictively powerful, probably best explains Third-World revolutionary or pre-revolutionary conditions. Nor does he believe that a single global theory of revolution is likely to emerge: the principal intellectual barrier is the incapacity to reduce voluntarism (the factors of will, personality and chance) to order within the limits of indeterminacy. This part of the book is thoroughly documented by resort to almost all the existing secondary source literature. Followers of Tilly, Gurr, Huntington, Johnson and others, including those of dependencia writers like Bodenheimer, will find here a stimulating critique of their thoughts.

In the second 120 pages, Girling turns strongly condemnatory as he mercilessly flails U.S. follies in the Third World, such as our experiments with the electronic jungle battlefield, the cumulative effects of napalm bombings, or the techno-managerial approach of the counter-insurgency school. Social scientists engaged in such nonsense as Project Camelot or later efforts at rationalizing counter-revolutionary excesses come in for their share of the blame. This part of the book relies perhaps somewhat too heavily on secondary sources and occasionally on dubious press accounts. One might also wonder whether Girling is right in depending as much as he does on the pronouncements of American leaders as authoritative sources, although one is at the usual loss in suggesting what else the scholar might do, even if more of the secret record of policy making were at hand. Alas, U.S. actions in most of its interventions in the Third World accord well with the globalistic pronunciamentos set forth in the speeches of Washington policy makers.

The heart of Girling's thesis, which I acccept wholeheartedly, is that globalism does not work. This is so whether the global approaches are conceptions "from above," as in the containment policies of the earlier postwar administrations, or the interlinked credibility or modified balance-of-power policies of the later ones; or manipulations "from below," as in the specific (overt or covert) actions employed to bolster faltering elites in Third-World countries or to reduce the strength and appeal of their present or potential revolutionary opponents. The "push effect" that results from promoting the global environment in the direction of a world order desired by the U.S., one presumably conducive to enhancing fairly narrowly perceived U.S. security and economic interests, combines with the "pull effect" exerted by generally unpopular, unstable and threatened Third-World elites to draw the superpower into successively escalating stages of involvement. These produce agonizing and morally flawed interventions, which ultimately founder on their incompatibility with the democratic American domestic value structure. Such was certainly the case in Vietnam, as Girling well demonstrates, but he also documents an almost endless list of similarly escalating engagements and involvements elsewhere, from the Middle East to Chile, from Cuba to Thailand and Africa. The alternative to globalism is strategic disengagement (in the mode envisaged by Earl Ravenal) and a more particularistic approach to the Third World and its turmoil. But Girling is not sanguine that it will soon be adopted by the U.S., and he sees the other superpower engaged in a roughly similar search for global security and power, equally troubling to the prospects of world peace and socioeconomic justice in the Third World.

Girling forecasts a world of growing instability and fragmentation, providing "greater—not less—opportunity and 'necessity' for patrons to intervene in the troubled affairs of clients in order to safeguard their (patrons') interests from the threat of a takeover by rival interests. And there will be more rising patrons—not less—with both the capacity and the will to intervene" (p. 207). Short of a radical transformation of the perceptions of a desirable world order, unlikely to be achieved by either superpower, Girling finds the prospects grim indeed. And in this, I am forced to agree with him.

PAUL M. KATTENBURG

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Territorial Asylum. By Atle Grahl-Madsen. (New York: Oceana Publications, 1980. Pp. xvi + 231. $28.00.)

At this writing, issues of asylum are reported regularly in the newspapers to inform readers of every walk of life of the suffering of their fellow man. Thousands of Cubans jeopardized their future if exit were not granted by Castro, and a refuge offered in countries willing to take them. Asian boat people continue to leave their homes in the hope that some country will offer them asylum and life. There is the daily exodus of Chinese who try to find temporary or permanent asylum in Hong Kong. Many Haitians try to escape their homeland in pursuit of a safe haven. During the Moscow Olympics, stringent security measures were taken to curtail any opportunity of Russians and foreigners to seek asylum.

Against this background, I have read and reviewed *Territorial Asylum* by this distinguished Swedish scholar whose works include his magnum opus, *The Status of Refugees in International Law* (Sijthoff, 1966). This latest book provides those interested and working with the subject a valuable study on the present state of the law of asylum.

In the first 43 pages, Atle Grahl-Madsen presents an abbreviated account of the concept of asylum, existing rules on the right of states to grant asylum, an account of the municipal law on asylum as practiced in 15 states, and non-extradition and non-refoulement principles in international law. Grahl-Madsen ably discusses the contents to be included in any convention on territorial asylum; analysis involves potential provisions and their impact in regulating interstate relations and effects on the rights and duties of individuals.

After a brief review of the reasons for the failure of the 1977 United Nations Conference on Territorial Asylum held in Geneva, Grahl-Madsen presents his thesis for a protocol relating to territorial asylum. As he concedes, this approach may not bring international refugee law a great leap forward, but it is a step in the right direction. I find the author's analysis and suggestions for the proposed protocol balanced and well thought out.

The remainder of the book contains many documents pertinent to the subject—that is, all agreements, treaties, resolutions and international expressions, as well as individual drafts of a convention and protocol. Aside from the obvious merit of having many of these references in one book, the differing viewpoints in the drafts can be studied and left for further individual reflection and argument.

A list of abbreviations, index and bibliography are helpful to the reader. With respect to the bibliography, the author includes only a partial list from his earlier opus aforementioned. This work stands on its own merit and the bibliography could have been easily expanded to assist the researcher.

DANIEL C. TURACK

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The Western Alliance: European-American Relations Since 1945. By Alfred Grosser. Translated by Michael Shaw. (New York: Continuum Publishing Corp., 1980. Pp. xxiv + 375. $19.50.)

The time is ripe for a fresh look at the Atlantic Alliance. The economic crises of the 1970s and the uncertainties about American leadership have dated the definitive studies of the 1960s. Alfred Grosser, whose work on French and German foreign policy is unsurpassed in Europe, is a logical choice to tackle the complexities of the past decade.

A new analysis of European-American relations must disentangle continuity from change. While American analysts have concentrated on changes in Atlantic relations and particularly the diminished position of the United States, Grosser stresses continuities. The realities of mutual dependence for security, a commitment to pluralist regimes and economic interdependence dictate a loose alliance framework binding Western states together. However, competing perceptions of those realities held by leading actors guarantee a legacy of alliance disagreement and friction. National perspectives are a product of postwar identity problems. Britain and France had to adjust to the loss of empires; Italy had to cope with internal conflicts and cleavages; Germany sought to recover both its European and international legitimacy. Grosser is at his best when examining alliance problems from different national points of view.

Unfortunately, Grosser is not always careful to distinguish between problems of perception and conflicts in interest. The German desire to base European security on nuclear deterrence and the

American preference for a more flexible conventional and nuclear response is a difference based not on misperception but on conflicting interests defined by geography. One suspects that interest rather than perception (while the latter may influence the former) will prove the more effective analytical tool in any full accounting of alliance differences.

To stress continuity is to minimize change. For Grosser, the only important change is change in alliance structure. That structure changed only between 1947 and 1949 when Germany and Italy joined with the West and the Soviet Union shifted from ally to common enemy (p. 322). By defining change in this way, Grosser misses the opportunity to explore and explain the many changes that have taken place within the existing alliance structure. Because Atlantic boundaries have always been broad and flexible, changes that have taken place within the framework of the alliance often have been as important as changes in the framework itself.

Alliances, like constitutions, are the product of time and place. Their long-term usefulness is determined by the ability to adapt to a changing world. It is this rich legacy of adaptation that Grosser fails to address. In the 1960s and 1970s, three challenges have prompted extensive adjustment. In security affairs, the slow and uncertain accommodation with the Soviet Union and Eastern Europe has made security both less important and less manageable in alliance politics. What dominated the alliance agenda in the 1950s must now share top billing with economic affairs. While Washington prefers bilateral diplomacy with Moscow over SALT and multilateral negotiations to reduce conventional forces in the heart of Europe, alliance partners feel excluded from strategic discussions that affect them most and resent MBFR negotiations that serve as a reminder of Washington's flagging European commitment.

A second challenge has grown from the economic structure of the alliance itself. The liberal trade and monetary regimes have forged an economic interdependence in the 1970s quite unlike the dominant-dependent relationship of the 1940s. The interlocking of economies has followed the reduction of barriers to trade, free convertibility of currencies and the extensive foreign activities of banks and corporations. However, the management of this interdependence has been complicated by the active intervention of pluralist governments in their national economies. All too often, the demands of domestic economies have clashed with the constraints of interdependence. Where domestic priorities have prevailed, as has so often been true in the United States, Atlantic crises have followed. The attention given to the management of economic matters in Atlantic relations is the major change of the 1970s and as such receives far too little attention from Grosser. Clearly, he is uncomfortable with matters of money and trade which are squeezed in where chronologically appropriate but without sufficient background and explanation.

The third challenge to the alliance, the quadrupling of oil prices in 1973-74 affected the alliance in two important ways. It demonstrated a new American vulnerability to international economic forces that reduced an important alliance asymmetry that had permitted the Americans a posture of "benign neglect" in the late 1960s and early 1970s. Moreover, it internationalized those economic affairs that had been the exclusive domain of the Western Alliance. Not only OPEC but other Third World States and Japan now participate actively in negotiations to solve common problems. Grosser's neglect of these alliance dynamics makes *The Western Alliance* a book much like those that have preceded it. While its four parts treat the 1940s, 1950, 1960s and 1970s in turn, the emphasis is on the first two decades. Grosser handles this early material with a steady hand but, except for some interesting personal insights, there is little that is new. In parts 3 and 4, where there is considerable potential for new insights, the work is much less carefully done. The organization is unclear, the criteria for including some topics and excluding others is never made explicit and analysis seldom moves beyond impressions.

Sorting out the complexities of Atlantic relations is not an easy task. Simple generalizations are inadequate where security, economic and political affairs increasingly mingle and mix. Grosser has given us a useful rendering of the realities of the 1950s but not the new synthesis of the 1970s.

THOMAS L. ILGEN

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Regionalism Reconsidered: The Economic Commission for Africa. By Isebill V. Gruhn. (Boulder, Colo.: Westview Press, 1979. Pp. xiv + 154. $20.00.)

What function should regional organizations perform in the years ahead? Isebill Gruhn, noting a decline of interest on the part of writers in regional integration as theory and as process, argues strongly and cogently for a redefinition of Economic Commission for Africa (ECA) activities to bring it into line with the "developmental" concerns of governments. Gruhn's thesis is clearly stated: The earlier focus of African-inspired region-builders upon the transfer of resources to achieve economic development deals inadequately with the equally imperative needs for the manpower skills, institutions, and infrastructures "to convert capital into human welfare and societal development" (p. 26). Although Gruhn gives little background evidence to support her assertions on the earlier work, her general point on directing ECA's attention toward the facilitation of African regionalism is well taken. In particular, Gruhn's linking of ECA's regionalist role to the current effort to fashion a New International Economic Order (NIEO) gives this volume considerable policy relevance.

Gruhn's account of ECA organizational failures in the 1960s and 1970s explains much of the reason for Africa's overall lack of interest in this international agency. A variety of factors are set forth to describe ECA's inability to gain an important place for itself in the new African order: its insufficiency of focus and purpose, limited organizational capacity, low budgets, lack of African identity, dependence on foreign donors and personnel, and poor coordination with African governments.

Yet, for all these deficiencies, Gruhn stresses the need to remain fully apprised of the organization's contributions. These accomplishments are grouped under three broad headings. First, the ECA functioned as an arm of the international development assistance community. It generated and distributed data, established national statistical services, organized data banks, trained administrators and skilled personnel, and provided consultancy services. Second, the commission acted as a facilitator of interstate integration. It arranged inter-African conferences and contributed research and preparatory work leading to the establishment of such organizations as the African Development Bank and the Economic Community of West African States. And third, the commission set up its own subregional offices (most notably, the Multinational Programming and Operational Centers) to deal with interstate activities, and, ultimately, to serve as focal points for further subregional integration. For Gruhn, ECA's second role was its most significant, and, when outlining a program for ECA reform, she contended that it was the area which could most profitably be emphasized.

This brings us to the linkage envisaged between ECA and NIEO—the most intriguing aspect of the book. Gruhn—correctly, I feel—depicts ECA as a low-profile body, lacking focus as well as African roots. By tying its fate to NIEO, especially with respect to its integrationist role, she seeks to breathe new life and purpose into this agency. *Regionalism Reconsidered* thus changes tone toward the end, moving from analysis to prescription. "In short," she writes, "the time is now ripe—which it was *not* in the '60s—for African states to make arrangements that facilitate their interaction and allow them to maximize the utility of limited resources" (p. 119).

To grant that the time is now right for African integration efforts is not to demonstrate conclusively that this was not the case before. Certainly the advent of NIEO has altered the context within which regional integration must be discussed at this juncture—and Gruhn's insights are helpful here—but generalizations on earlier processes deserve an equally careful examination against the context of their times. This point is not meant to detract from the usefulness of this work, but mainly to express a wish for more—more about the realistic choice options of the past, more about the environment of contemporary choice, more about the decision processes at both the state and interstate level, and more about the ECA as a part of the larger United Nations operation. Isebill Gruhn deserves credit for moving us forward, but much remains to be written on this topic.

DONALD ROTHCHILD

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Why the Vietcong Fought: A Study of Motivation and Control in a Modern Army in Combat. By William Darryl Henderson. (Westport, Conn.: Greenwood Press, 1979. Pp. xxix + 163. $17.95.)

Why the Vietcong Fought is a narrowly focused examination of the organizational strengths of the Vietnamese People's Liberation Army (PLA) which enabled it to endure under the weight of massive American firepower introduced into Vietnam from 1965 onward. The author, a former U.S. Army battalion commander in Vietnam, undertook this study to discover why the PLA was able to endure, maintain its cohesion and remain on the battlefield while the U.S. Army was showing signs of the disintegration of small-unit cohesion.

The main sources for Henderson's study consist of 61 K-series interviews of PLA defectors and prisoners conducted by the RAND Corporation in 1965-67 and captured Vietnamese documents published as part of the *Viet-Nam Document and Research Notes* series. The K-series of interviews, named after the project's director Konrad Kellen, forms part of a 20-project study, totaling 2,400 interviews, undertaken by RAND. These materials were supplemented by the author's personal experiences and observations which include, most interestingly, private discussions with Warsaw Pact officials (p. 24).

Building up on this core of primary material, Henderson also incorporates the work of other specialists such as Douglas Pike (on the Vietcong) and Alexander George (on the Chinese PLA). There is one important omission, however. Although Paul Berman's 1970 Ph.D. thesis ("The Liberation Armed Forces of the NLF: Compliance and Cohesion in a Revolutionary Army") is listed in the bibliography and mentioned briefly in the text (where it is curiously termed a "recent study" on p. 96), no mention is made of Berman's book, *Revolutionary Organization: Institution-Building Within the People's Liberation Armed Forces* published in 1974. This omission is unfortunate because Berman's work, which also draws on the RAND interviews, is the most comprehensive and analytically satisfying account yet published.

Henderson attempts to answer the question why the PLA fought so well by examining the relationship between the three-man cell, the primary unit of the PLA, and the larger PLA organization, which he terms the secondary group, at the squad and platoon levels. In discussing the linkage between these two groups, Henderson rightly stresses the crucial role of the cadre-leader. The cohesion of the three-man cell is attributable to various factors: the cell's ability to cater to the material, social and psychological needs of its members, the formation of a "buddy relationship" among its members, and the fact that surveillance, control and criticism/self-criticism functions are performed within the cell.

The position of the cell within the PLA is also conditioned by various characteristics of the secondary group. Henderson discusses six cultural variables: nationalism, group orientation, orientation towards hierarchy, "face," status and fatalism. The role of the cadre-leader is crucial in linking the three-man cell to the larger organization. Henderson's discussion of the cadre-leader, which occupies over a third of the text, constitutes his main contribution to our understanding of motivation and control within the PLA. To quote Henderson directly,

> The cadre-leader, by linking the primary group to party/PLA goals, was seen as crucial to the accomplishment of the overall success of party/PLA goals, was seen as crucial to the accomplishment of the overall success of party/PLA objectives in South Vietnam. In order to succeed in his three roles of party agent, primary group leader, and chief of surveillance, the cadre-leader had to successfully: (1) transmit party goals to the three-man cell; (2) be dominant within the three-man cell and above; (3) conduct surveillance of individual and group performance to detect deviance and insure compliance with party/PLA goals; and (4) indoctrinate "good communist soldiers." To accomplish these functions, the cadre-leader drew upon the bases of power described [i.e., "reward," "coercive," "legitimate," "expert" and "referent" power] and exercised certain control devices such as criticism, surveillance, and isolation (p. 121).

During the 1965-67 period this system basically worked, according to Henderson, as "there was no widespread disintegration and there was no mass defection of platoon-size or higher units" from the PLA (p. 117).

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The Third World Reassessed. By Elbaki Hermassi. (Berkeley: University of California Press, 1980. Pp. 223. $15.00.)

Elbaki Hermassi, a sociologist, argues that "no serious attempt has been made to distinguish the sociological study of social structure from the sociological analysis of development" (p. 10). To promote the latter, he has written this series of essays, much of the validity of which comes from his own Third-World background (Algeria). The writing is stimulating and expository in form, designed to challenge Western theories of development. Instead of a new theory, however, Hermassi calls for inductive thinking "which puts theories on their heads" (p. 12). Although he denies having a paradigm, he seems to be guided by raw empiricism in which every datum seems equal to all other data.

Hermassi devotes much attention to the psychology of the alleged trauma produced by colonialism. Because of psychic trauma, he sees basically pathological societies which have little to offer their people. This perspective, he argues, flies in the face of Western scholarship which he consequently rejects by questionable generalizations in a work which otherwise condemns generalizations. Europe in its colonial domination "did not hesitate to destroy any form of humanity in order to shape the world in its own image" (p. 2). Western scholars "have failled to realize that no problem can be studied apart from its relations to others" (p. 13). Westerners think in terms of Progress, Evolutionism and Development, which guide modernization theorists on one hand and the neo-Marxist theorists of dependency on the other. Each fails to see that change is dependent on a "unique constellation of highly disparate events" which "can thus hardly be amenable to paradigmatic thinking" (p. 12).

Hermassi's corrective is a sociological analysis of development. Such an analysis need not destroy "Western concerns for rigor and validity" but would add an awareness of "Third World sensitivity to relevancy and immediate problems" (p. 12), as well as a "world-historical" perspective (p. 43). A sociological analysis would reveal that Third-World societies are divided societies; that they are not changing uniformly; and that in reality no development has occurred, whether the elites use self-reliance, capitalism, socialism, or international co-optation—"everything is still just about as it was" (p. 174). Furthermore, we must now reject the "catching-up" fallacy (p. 192) and recognize that the Third World can never be a high-productivity, high-consumption area. Knowing these things requires us to shift our research on Third-World societies from the general approach to the study of specific societies and "the particular historical conditions under which they confront the processes of national formation and transformation" (p. 200).

Despite these conclusions, Hermassi believes that something called development is possible to a limited extent (pp. 177, 185). He argues that the lot of Third-World peoples can be improved if the rich countries change their life styles (p. 192). Conversely, he also argues that "it is futile for Third World countries to blame the international system for their own troubles and to keep postponing their assumption of full responsibility for their own development" (p. 92).

This book is filled with provocative and often heuristic comments, as well as some infuriating ones. However, it will frustrate many political scientists. Although he argues for the "centrality of politics" and the "primacy of the state" (p. 201), he does not deal at all with the stuff of politics—who gets what, how values are allocated, what structures and functions are important, how political institutionalization occurs, the resolution of political crises, governors versus governed, or the meaning of *political* development.

Another frustration I felt stems from the fact that a "world-historical" perspective, which the author admits requires a long-term view, is missing from the essays themselves. A long-range view tells us that in all Great Revolutions (technological at their base) there will be cultures which come in last, that are furthest from the characteristics which led to the original changes, that are most resistant to change, and that are forced to change as secondary adapters rather than pristine ones. A long-range view tells us that societies change, and a mere generation is insufficient time to say what Third-World countries can or cannot do.

Finally, Hermassi's insistence that Third-World development should be studied without reference to the rest of the world is made without his ever explaining how developing societies (even as he sees them) can be evaluated without at least some notion of what developed ones are like.

CLAUDE S. PHILLIPS

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The Fall of South Vietnam: Statements by Vietnamese Military and Civilian Leaders. By Stephen T. Hosmer, Konrad Kellen and Brian M. Jenkins. (New York: Crane, Russak, 1980. Pp. 267. $14.50.)

This report summarizes oral and written statements by 27 former high-ranking South Vietnamese military officers and civilians about their perceptions as to the reasons for the collapse of South Vietnam. While they found much to criticize in their own behavior and institutions, the dominating factor in the debacle was the shifting role played by the United States in Vietnam.

The report focuses on the later stages of the war, first reviewing the period ushered in by the January 1973 Paris Agreements and then dealing with collapse, beginning with the loss of Phuoc Long in III Corps north of Saigon in early January 1975.

One major complaint was that the United States had saddled them with a military organization and tactics which were ill suited to combat the North Vietnamese and could not be maintained after the United States withdrew its forces and cut back the level of its military assistance.

The Americans were also criticized for the related Vietnamization program, both for starting it too late and for attempting to train the Vietnamese army in the image of the American forces. Initially, the Americans tended to push their allies aside, convinced they had all the answers. Only later did they attempt to put the conflict burden on the backs of the Vietnamese.

The Paris Agreements were also cited as a major source of the debacle, particularly these disadvantages:

> (1) the in-place cease-fire, which made its area of control vulnerable to Communist "land grab" tactics and made military difficulties for the GVN; (2) the fact that the Agreements left North Vietnamese troops in South Vietnamese territory; and (3) the establishment of a Committee of Concord and National Reconciliation, which gave the Communists too much legitimacy (p. 31).

They also were disturbed by the tough pressure exerted by President Nixon on Thieu to sign the agreements. They found even more pernicious the fact that violations of the Agreements were tolerated by the United States.

The South Vietnamese officials also perceived an almost callous abandonment of South Vietnam, particularly during the 1974-1975 North Vietnamese offensive. Paradoxically, this was mixed with their misplaced confidence that the United States would "do something" if the enemy seriously endangered their national existence.

These perceptions have been endorsed by responsible Americans, including U.S. Defense Secretary Melvin Laird (in "America's Principled Role in World Affairs," in *Detente and Defense,* ed. Pranger, American Enterprise Institute, 1976):

> As a member of Congress, I criticized the Americanization of the Vietnam War. As secretary of Defense, I had to win support from Congress for the de-Americanization of Vietnam through the initiation of novel assistance measures.

Concerning U.S. abandonment of South Vietnam in 1975, Laird stated:

> America's convenient neglect of its solemn pledge to guarantee the security of South Vietnam cannot be excused. No long-term goal was served by ignoring the enforcement of the Vietnam accords by shrugging off arms build-ups prior to the North Vietnamese offensive of 1975.

As ambassador to Thailand in early 1975, I saw pictures showing the Ho Chi Minh Trail being transformed into a major turnpike on which vast military stores were being moved south for the final assault. I had to justify to the Thai government the continued presence of B-52s and other combat aircraft on the Thai territory by claiming that the United States would strike if there were a major violation of the Paris Agreement by Hanoi. I knew such a response was highly unlikely. But the Thais as well as the South Vietnamese still believed we would not abandon what we had already spent in blood and treasure to defend.

The failure of Congress to appropriate the "supplemental funds" in early 1975 was the beginning of the end. Supplies of rice, petrol and ammunition began to dwindle. President Thieu ordered withdrawal from I and II Corps because of reduced U.S. assistance. Even with the best of troops a retreat in the face of an aggressive enemy is the most difficult maneuver to execute in war. It is no wonder the largely immobile South Vietnamese units failed this test and disintegrated in panic. The fall of Saigon became inevitable by early April.

The report concludes on this somber note:

> The destiny of South Vietnam, in the final analysis, was regarded to have rested in the hands of others—and in this sense was a matter of fate, which, after all, is a concept deeply embedded in Vietnamese culture. And, as one respondent concluded, "Fate was not on our side." (p. 200).

In retrospect, the American failure in Vietnam can be traced to the absence of cultural empathy and the arrogance of the "Best and the Brightest." A realistic evaluation of the American role in Vietnam is more than ever essential. This book will prove a useful starting point.

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Conflict and Violence in Lebanon: Confrontation in the Middle East. By Walid Khalidi. (Cambridge, Mass.: Harvard University Center for International Affairs, 1980. Pp. 217. $12.95, cloth; $6.95, paper.)

The Harvard Center for International Affairs has a distinguished history of publishing a high-level series of books on international politics and conflict. Unfortunately, CFIA's first—and one hopes last—misjudgment seems to be the publication of Walid Khalidi's *Conflict and Violence in Lebanon,* which is no more than a political pamphlet and tract written by an obvious propagandist and advocate.

The fact that the author is a Palestinian professor of political science at the American University in Beirut does not necessarily lend respectability to his work. The book on the conflict in Lebanon contains 122 pages (minus appendices, chronology and glossary), but only 20 pages actually deal with the Lebanon Civil War, another 24 pages with the actors in the conflict and only four with the Palestinian actors. By contrast, there are over 50 pages dealing with the hostilities and the aftermath of the Israeli invasion of March, 1978.

It is a fact that one of the key elements in the problems and struggles of Lebanon is the emergence of the Palestinian Liberation Organization as a power broker and as one of the chief destabilizing elements in Lebanon. Yet the PLO's emergence in Lebanon, its motivations, its military and political strategies and its aims are barely mentioned, and certainly are not analyzed or explained in depth. It is a remarkable sin of omission akin to writing a history of Islam without mentioning Muhammad.

The theme of Khalidi's extended political pamphlet is that the real nemesis of Lebanese stability are not the Palestinians but the Israelis, whom he paints as dedicated to destroying Lebanon and bringing about partition. It should not be surprising that Khalidi, a Palestinian intellectual and academic tainted by three decades of anti-Zionist propaganda and warfare, should take this turn in his book. It would have been much more profitable for CFIA to commission a younger, new-generation Palestinian to write a penetrating study of the Palestinians, their problems, aspirations and goals, rather than waste good paper on this peculiarily hollow and limited work.

The major chapter in Khalidi's book focuses on the Israeli invasion of Lebanon. This chapter is tendentious, inaccurate and rife with sins of omission and commission. The author contends, "One specific political objective of the Israeli strategy would seem to have been the disruption of the Shtaura accord of July 1977, and therefore the entire infrastructure of the Sarkis-Syrian PLO consensus on the interpretation and implementation of the 1969 Cairo Agreement. . . . The Shtaura accord had at least the merit of imposing a freeze on Palestinian operations across the border. Given its centrality to the slowly emerging Lebanese consensus on national reconciliation and the reconstruction of a national army, as well as its vital importance in the whole range of Lebanese-Syrian-Palestinian relations, the Israeli objective can only have been to throw everything into turmoil again" (p. 126).

This assertion is obviously off the mark and not proven. If the Israelis wished to "throw everything into turmoil again," their strategy would not have been the conquest of southern Lebanon but the annihilation of the PLO's army, a strategy that involved Syria in the North. The author reluctantly admits that the tacit Israeli-Syrian alliance helped to cripple the PLO in 1978.

Khalidi has difficulty in making up his mind just what the Israeli strategy is: containing the PLO, containing terror in Israel, the conquest of South Lebanon, the partition of Lebanon or a response to the challenge of the Syrians. One has to assume that these cannot be complementary goals and not all of them can be Israeli goals. It should be noted that Israel is interested in a stable Lebanon, replacing its conflict with Syria with peaceful borders, not because it holds great love for Lebanon, but because it wants nothing to do with a fourth front on its borders. Thus, it is because of the tacit Israeli-Syrian bargaining that Lebanon is still intact and in one piece. Khalidi politely dances over the implication that Lebanon is in fact, if not formally, under Syrian military protection and hegemony.

Khalidi peremptorily dismisses the Syrian role in the Lebanese conflict in less than nine pages. And, without evidence, and contradicting himself, he arrogantly imparts a great deal of responsibility to the United States.

Conflict and Violence in Lebanon is not a study of conflict and violence in Lebanon. It is clearly a pamphlet, bewailing Palestinian woes, decrying Machiavellian U.S. strategies, and belaboring the arch-villanous Israelis. There is probably a lesson to be learned here, not because of what Khalidi has written but because of what he has *not* written. The lesson demonstrates the need for an authoritative, insightful, and impartial study of the real conflict in Lebanon, a study that will include an incisive look at the PLO presence and tactics in Lebanon. What Khalidi has accomplished is to point out the dearth of information and the large amount of misinformation that exists in this area of study.

AMOS PERLMUTTER

American University

The Origins of the Cold War in the Near East: Great Power Conflict and Diplomacy in Iran, Turkey, and Greece. By Bruce R. Kuniholm. (Princeton, N.J.: Princeton University Press, 1980. Pp. xxiii + 485. $27.50, cloth; $10.50, paper.)

These are fitting days in which to give heed to an expert rehearsal of the action in the Near and Middle East theater at the onset of the First Cold War. Bruce Kuniholm provides that record in a tome of vast learning and detail. It is diplomatic history written mainly for diplomatic historians, a massively footnoted reference work more than an analytical synthesis. But there is an awesome wealth of evidence here available also for others to mine about the origins of containment.

Kuniholm's own approach is post-revisionist, nonpolemical. He rebuts revisionist interpretations of his specialty (Greece, Turkey, and Iran) but readily concedes utility to much of the revisionist writing about other (especialy later) cold-war conflicts. His principal theme is "the fact that American policies [in the region] emerge from a framework of traditional European power politics, [and] traditional interests along the Northern Tier. . . . American-Soviet rivalry gradually supersedes the earlier Anglo-Russian rivalry in the region" (p. xvi). Kuniholm provides background on hundreds of years of Czarist expansion and the Great Game between Russia and England and treats all of World War II thoroughly; likewise, his ensuing coverage of 1945-46 Iran, Greece, and Turkey is impressively searching and professional. To be sure, 1947—the Truman Doctrine itself and the Greek-Turk aid program—gets skimpy treatment by comparison; but that can be somewhat excused by the greater availability of other monographs on that particular crisis.

What remains troubling is the rather uncritical ease with which Kuniholm glides into the view that "the policy of containment in the Near East was a realistic and pragmatic policy. . . . If one were to find fault, it would have to be in its rationalization [ideologization, globalization], in the legacy of that rationalization, and in analogies engendered by the policy's success in the Near East" (p. xx). Kuniholm's own assumptions are matter-of-factly geopolitical. Americans came to "recognize" their country's interests in the region as being "synonymous" with Britain's (p. 382); the resulting policy was "well-founded" (p. 431). He assigns the credit mainly to State Department careerists. (Perhaps the availability of documents skews the evidence in the latter direction, but it remains impressive—though regrettably not balanced by any thorough, respectful treatment of Congress and public opinion; even the presidents get rather short shrift whenever it appears that their own naive idealism or domestic political considerations might have been delaying their acceptance of geopolitical imperatives.)

Kuniholm apparently sees two of these imperatives as ultimate. In my view he presents them too uncritically. He accepts without much question (a) that America did need Middle Eastern oil and (b) that, but for containment, communist pressures (internal and/or external) would have thoroughly destroyed Greece, Turkey, and Iran as buffers on the north.

Kuniholm is able to prove convincingly that the pressures were intense. Yet in Greece (as he admits) the Russians were generally honoring their October 1944 spheres-of-influence deal with Churchill (even if the Yugoslavs, Albanians, and Bulgarians were not). And in Turkey what the Russians were demanding was eastern provinces that had already been theirs from 1878 to 1921, plus bases at the Dardanelles. Moreover, in Iran, they "would undoubtedly have accepted a division of [the country] into spheres of influence" (p. 204); the British of course were peaceably retaining anyway their own sphere of influence in the oil-rich south. Kuniholm's sense is that Turkey and Iran could not have survived the Russian nibbles without becoming Soviet satellites; hence they would have been fully eliminated as buffers for the oil of Iraq and the Persian Gulf. This may be true (or perhaps Soviet-American friction over rump regimes would have become a major aggravation in the impending cold war anyway)—but alternative scenarios deserve more consideration than Kuniholm offers.

More fundamental is Kuniholm's neglect to spell out with sufficient thoroughness just why Persian Gulf oil was considered so crucial for America in the mid-1940s and why he concurs in this opinion: was it directly crucial, or just indirectly because it was already so important for Britain and Britain was important for America? Was it crucial to have the oil ourselves, or to keep Russia from having it? If the latter, did we aim to keep their economy weak, or to ensure that they would never have a spigot to turn off against us? And was the implicit objective one of forestalling the natives, not just the Russians—i.e., preserving cheap royalties, not just access per se? How many of these arguments were made justifiably, at the time? Thirty years later one could see clearly that the OECD countries were enabled to float onward to prosperity in the 1950s and 1960s on a sea of cheap Middle Eastern and North African oil—yet in the process they all became excessively addicted, much more so than even Britain had been in 1945. Today the withdrawal symptoms have become so agonizing that the mid-1940s definition of America's national interests as including "friendly," buffered governments along the Persian Gulf—whether wise or unwise then—has become self-fulfilling and hence is probably wise today. But its being wise today does not make it wise then. These are difficult questions that one wishes Kuniholm had pondered in his otherwise masterly book.

H. BRADFORD WESTERFIELD

Yale University

Latin America, the United States, and the Inter-American System. Edited by John D. Martz and Lars Schoultz. (Boulder, Colo.: Westview Press, 1980. Pp. xvi + 271. $24.50.)

This collection contains original essays—several coauthored—by 14 social scientists, predominantly from political science, all of whom were either students or colleagues of Federico Gil at the University of North Carolina, Chapel Hill. The book is intended to honor Gil and the contributions he has made over the years to the understanding of Western hemispheric relations. Toward that end, the collection succeeds reasonably well. The essays are intelligent and thoughtful, in a contemplative and sometimes analytic manner. But as a group the essays simply don't constitute a coherent, let alone systematic, survey of the subject. The topics, while often timely, seem arbitrarily selected, ranging so broadly in subject matter, scope, and not least of all, style and organization, that it is difficult to imagine any reader finding the collection totally useful or even interesting.

The editors make a valiant effort in the introduction to identify four basic themes: the dominance of the United States, the role of humanitarian values (e.g., questions basically of human rights), the critical importance of economic variables, and something they term "the more things change. . ." (p. 12), which essentially translates into the familiar ebb and flow of U.S. interest in Latin America. These themes do indeed appear throughout the collection, but rarely in organizationally separable or conceptually distinct ways. What seems to emerge is the perhaps obvious assertion that understanding the inter-American system requires a knowledge and sensitivity to Latin America and the United States, the values and perspectives which separate these regions, and the reactions within each created by their historical relationships.

The first of the three sections of the collection, titled somewhat amorphously "Perspectives on Inter-American Relations," addresses some of the traditional and contemporary overviews (paradigms) of inter-American relations, specifically a liberal-developmental model, dependency, and what Steven W. Hughes and Kenneth J. Mijeski in their chapter call "organizational ideology," best represented according to them by "the preliminary efforts of Guillermo O'Donnell" (p. 25). They conclude by arguing for tolerance and scientific pluralism, and, acknowledging their debt to Paul Feyerabend, "methodological anarchism." John S. Gitlitz and Henry A. Landsberger provide a workmanlike and considered review of dependency as the concept has been applied to Latin America, again urging restraint and moderation, and admonishing its advocates to consider, "Is evil perhaps not a good deal more widespread than capitalism?" (p. 69). Gustavo Lagos and Alberto Van Klaveren point toward a diversification of Latin America toward closer relationships with Europe, Japan, and Third-World Nations, and less dependence on the United States.

The second section of the book, "United States-Latin American Relations: The Shape of Hegemony," contains a useful if somewhat elementary chapter by Kenneth M.. Coleman ("The Political Mythology of the Monroe Doctrine: Reflections of the Social Psychology of Hegemony") and a discussion by John D. Martz of the important if sometimes implicit conflicts between Latin America and the United States in interpreting the idea of democracy and its underlying values. The section also contains an excellent review by Enrique A. Baloyra of Cuban-U..S. relationships in the seventies, a good, old-fashioned blow-by-blow analysis followed by some provocative questions regarding future relations between the two countries.

The third and final section of the book presents three chapters dealing contemporarily and historically with U.S. attitudes toward human rights in Latin America, including a detailed account by Joseph S. Tulchin of the impact of U.S. human rights policy in Argentina. Collectively, these chapters identify many of the dilemmas resulting from current U.S. policies, and the important historical antecedents that promoted them.

Had this book gone through another editorial draft, with greater attention to matters of organization and style and more integration among the contributions, it would probably have been improved to the extent possible for a project of this kind. I assume the exigencies of publishing the volume precluded such revision. If the result is less than ideal, the book nevertheless makes some useful if random contributions.

RONALD H. MCDONALD

Syracuse University

Threats, Weapons, and Foreign Policy. Edited by Pat McGowan and Charles W. Kegley, Jr. (Beverly Hills, Calif.: Sage Publications, 1980. Pp. 324. $20.00, cloth; $9.95, paper.)

This volume, dedicated to the memory of John Gillespie, is the fifth in the series of Sage International Yearbooks of Foreign Policy Studies. It is the second in the series which has focused on a specific aspect of foreign policy and, like all others in the series, it seeks to maintain a comparative perspective.

The editors wish in this volume to bridge the gap between "scientific foreign policy studies" and "national security/defense studies." To do so, they bring together nine articles and a bibliographic review by a selection of analysts at research corporations in the Washington area and academics. One of the interesting features of the book is the consistency of methodology and substantive interests of the two author subsets—it makes one almost wonder exactly what gap is being bridged. The pieces almost uniformly reflect serious scholarship and make important contributions to our knowledge.

The chapters fall fairly clearly and are placed into three sections: threats and foreign policy, weapons and foreign policy, and modeling arms races. Although there are clearly other topics of interest at the intersection of foreign policy and defense studies, such as various aspects of military conflict, disarmament negotiations, or policymaking processes, the subjects chosen are of great importance and interest. It is good that the editors have somewhat restricted the scope of issues examined because it does allow some comparison and contrast of articles.

As in all edited collections, the contributions vary in quality and impact. Articles by Farid Abolfathi on U.S. military spending and public opinion, by Charles Kegley, Gregory Raymond and Richard Skinner on bases for nuclear armament decisions in various countries, and by Michael Wallace on superpower arms spending will be of special interest to many; they seek to bridge a more important gap than that noted above, but one which is only touched upon by the editors, namely that between scientific research and policy relevance.

That point brings us to one of the least satisfactory aspects of an otherwise well-selected set of solid, scholarly pieces. After finishing the collection, presumably most readers are going to ask exactly what they know that they did not know before they began. A variety of individual insights and conclusions come to mind, but any organization of them will need be provided by the reader. The editors can hardly be expected to provide a theory of foreign policy. They could, however, have helped the reader to collect the individual insights and conclusions and to sort them in terms of theoretical and/or policy implications. The volume is considerably coherent in methodology and subject matter, which is not always true of edited collections, and provides a base upon which one could build further.

More generally, it appears to be time in the life of this valuable series for the editors to step back and review progress. That review should definitely not be in terms of the number of scientific and comparative foreign policy articles published in their volumes or elsewhere (as in their annual bibliographical essay) but in terms of the knowledge gained and systematized. By identifying a structure of learning the review could also go far in pinpointing weaknesses in it. This is, in fact, the direction in which the editors are moving, and perhaps the sixth volume will provide the needed review and overview.

BARRY B. HUGHES

Case Western Reserve University

Terrorism and Hostage Negotiations. By Abraham H. Miller. (Boulder, Colo.: Westview Press, 1980. Pp. xvi + 134. $16.00.)

This book, one in a series published by Westview on terrorism, is devoted to comparative analysis of past "barricade and hostage" situations, and the devising of effective strategies for coping with a feared spread of international political terrorism to American soil. Drawing on case materials and interviews with high-level decision makers in the U.S. and abroad who have been involved with past hostage episodes, it was written just a bit too early to incorporate the recent events in Teheran, London, and Bogota, which in combination might have provided valuable, albeit prescriptively ambiguous, additional lessons.

Abraham Miller's work is pegged at two somewhat distinct levels, seeming to reflect, respectively, his background as a professional political scientist and his recent experience as a visiting fellow with the National Institute of Law Enforcement and Criminal Justice. On one level, there is a wide-ranging though succinct discussion of terrorism as an aspect of contemporary international relations, ramifying into subjects such as: the role of ideology in defining terrorist movements, the UN's handling of the issue (it recognizes only Western "colonial" terrorism), terrorism as a stage of revolutionary warfare, the connection of terrorist groups to major power sponsors, terrorism as—in part—a creation of the mass media, and so forth.

With this as background, Miller then proceeds to focus more narrowly on the tactics applicable to coping with one type of terrorist operation, that involving the taking of hostages. The subject is competently handled, though at some points virtually on the level of a "how to do it" manual for "SWAT" teams which might, for better or worse, perhaps be of more interest to a police trainee than to a student of international affairs. Discussed here are recruitment and training of personnel, tactical procedures in hostage situations, the use of aerobics and isotonics in producing physically fit police assault squads, and the pros and cons of forming elite SWAT teams for counter-terrorist operations.

Miller is clearly concerned that the virtual immunity to international terrorist activities enjoyed by the U.S. up to now within its own borders may not last indefinitely, and that we must be prepared—politically and operationally—for the eventuality of our own "Maalot." Generally—and this is his central thesis—he asserts that the experience of Americans and others with *non-political* hostage situations (for instance, those arising ad hoc from ordinary bank robberies), the vast majority of which have seen happy outcomes, are more applicable than is commonly assumed to those with international dimensions. Using the bizarre Hanafi Muslim episode in Washington as a bridging case between the essentially political and nonpolitical, Miller recommends predominant reliance on protracted, patient negotiations in lieu of the more "macho" assault tactics which he notes have cost more lives than the actual killing of hostages by terrorists. In analyzing the frequent successes in such negotiations, he discusses the cumulative impact of the captors' weariness, development of "transference" between them and their captives, the ritual value (to the captors) of the negotiations themselves, and the assertedly normal absence of nihilistic suicidal intent by the terrorists.

As Miller is himself well aware, this prescription runs head-on into a contrary conviction—most tenaciously held in Israel but also by some in the U.S.—that a tougher "surrender or die" posture may *deter* still more frequent terrorist actions. Whereas he claims, however, that such an Israeli policy (and the formally stated U.S. position of "non-negotiation") have not in fact deterred further attacks, others might rebut that it has helped restrict the problem to manageable proportions; in Israel, such things have not yet seriously escalated. Miller should probably also have stressed that the Israeli and European (and American) situations are here fundamentally distinct. On the one hand, one is dealing with a real threat to a nation's existence; on the other, with the kind of spectacularly irrelevant, if sometimes deadly, mischief of a Baader-Meinhof Gang which, not surprisingly, produces vastly different perspectives on deterrence, the "expendability" of hostages, the long-range implications of capitulation to demands for release of prisoners, and so forth. More difficult to analyze is the question of whether the U.S., as a superpower and with a perhaps far less cynical and jaded public opinion than was true of France when it released Abu Daoud for oil and an arms deal, could so easily absorb the humiliation of capitulations which have come to appear near-routine in Western Europe.

One might wish that some other important themes merely bruited in this book had been elaborated upon—for instance, the effect of Arab oil leverage and of some terrorist groups' links to the USSR upon the outcomes as well as the origins of hostage situations. (The latter question is ably discussed by Charles Horner in "The Facts About Terrorism," *Commentary,* June 1980.) Then, too, Miller seems to nibble at the bare edges of a very touchy matter he presumably dares not make explicit, in gently taking the Carter administration to task for not regarding terrorism as a sufficiently serious problem, and for not taking the lead in combating it. In making this charge indirectly, Miller discusses the works of former ACDA Chief Scientist Robert Kupperman, who is portrayed as a voice in the wilderness, one apparently dismissed as an alarmist by administration leaders because of his stated anxieties about possible future terrorist use of weapons of mass destruction. Vaguely hinted here is the suspicion that the administration's low-key handling of the terrorist issue may be less a matter of its being perceived as a mere bagatelle than of its reflection of the administration's broader Middle Eastern policy concerns. Saudi Arabia, after all, is a major financier of the PLO, and the latter is also closely linked to Iraq and Algeria, two radical nations which have been quietly but persistently wooed by Washington in recent years. This book could, hence, perhaps have been enlivened by a franker discussion of the "Europeanization" of America's terrorism policy by analogy with the vaunted "Finlandization" of Europe.

ROBERT E. HARKAVY

Pennsylvania State University

India and Africa. Edited by R. R. Ramchandani. (Atlantic Highlands, N.J.: Humanities Press, 1980. Pp. xiii +314. $15.00.)

This volume contains 15 of the 17 papers presented at an All India Workshop organized under the auspices of the Centre of East African Studies at the University of Bombay in October 1977. Almost all the contributors are Indian academics, although there is some representation from India's diplomatic service. The papers are separated into three categories: (1) Historical and Political Dimensions; (2) Indians in East Africa; and (3) Economic Dimensions. All the papers, however, follow a similar theme: India and Africa are inextricably intertwined, reveal age-old associations, share a common imperial/colonial experience, identify with nonalignment, are eager to grow and prosper, and are determined to control their own destinies. The book is in some ways a clear and unequivocal statement that India is an emerging great power, and considers Africa, particularly sub-Saharan Africa, an important region in its expanding national interest. A complementary theme therefore focuses on the legacy of European colonialism and the more contemporary efforts by external powers, especially the United States, to influence and dominate the struggling people of Africa. There is throughout the volume either the explicit or implicit conclusion that India's relations with Africa are noble, salutary and beneficial, whereas the purposes and intentions of the Western nations are at best suspect and more than likely insidious.

Although the papers for this volume were prepared more than three years ago, no effort has been made to bring them up to date. Consideration is given to superpower activity in the Indian Ocean but the data base is very limited and the analysis thin and lacking in balance or objectivity. Mention, for example, is made of the conflict between the Somali Republic and Ethiopia on the Horn of Africa but little attention is given to the nature of the conflict. It is also surprising that Satyavati Jhaveri, who is so sensitive to a renewal of big-power maneuvering on the African littoral of the Indian Ocean, refuses to examine the role of Cuban forces in West and East Africa. To suggest, as he does on p. 90, that Castro was only interested in bringing hostilities between Ethopia and Somalia to an end and to induce the "contending areas to form a federation" is betrayed by subsequent events. The creation of a Marxist state in Ethiopia and infiltration of the Ethiopian government by Soviet, Cuban and East German "advisors" as well as the pressure on Mengistu Haile Mariam to fully communize the political system in the country indicates that the Kremlin and Fidel Castro were not simply interested in helping the Ethiopians overcome the "tyranny" of Haile Selassi. The author, however, sees no purpose in pursuing this matter. Rather, Indian concern over American activity on the Indian Ocean island of Diego Garcia is highlighted and given unusual prominence.

Appa Pant's study is similar. He welcomes the assertiveness of OPEC and implies that the industrialized countries at last will be forced to pay the price for their extravagance and "intolerable" burden that they have placed on the "world community" (p. 30). At no time, however, does he examine the impact of OPEC pricing policies on the petroleum-deficient Third-World countries. Moreover, his nonalignment statement does not even venture into an analysis of the reputedly nonaligned, such as Cuba, to carry forward the policies and programs of the Soviet Union, e.g., in Angola and Ethiopia.

Jayanti Patel's long article comparing the Indian political party system with African political party systems is perhaps the most scholarly contribution in the book. Patel takes extraordinary pains to explain how the Indian system was influenced by the European liberal tradition that rests on pluralism and why the Africans were more inclined to follow "Socialist" Europe, the belief in class struggle and revolutionary party organization. Africans more so than Indians, argues Patel, were required to resort to arms in achieving their independence. Thus they were more likely to "create parties with monolithic and penetrative character appropriate for launching an attack upon the colonial regime" (p. 133). Patel is hardly sanguine about Africa's chances for developing competitive party systems. The one-party system, the administrative state and the mobilization model appear to be more in Africa's future than India's more free-wheeling and open political experience.

R. R. Ramchandani has written an interesting article on the role and future of the Indian community in East Africa. He notes that the Indians went to Africa, essentially as indentured labor, to work on projects designed by the European colonizers. Many remained on and their community swelled so that at the time of independence for the peoples of East Africa they represented a notable, often important economic concentration. The black African population, however, did not look kindly on their Asian neighbors. In the years that followed independence, and no longer protected by European power, the Asians found themselves in the precarious position of neither being welcomed in Great Britain nor of being permitted to stay on in their African homeland. Their flight to India and the lukewarm, if not unsympathetic treatment in their historic country of origin is not one of India's noblest moments. Ramchandani attempts to explain this inhumane treatment of the Indians of East Africa. He notes the contributions to East Africa's development made by the Asian community but says the rampant xenophobia in the region negates their good works. Ramchandani cautions against retaliation in any form. India, he suggests, must ingratiate itself with East Africa and the larger continent. In this regard, India must view its overseas community as something of a liability. Indian assistance to East Africa, however, is not ruled out. In fact, the author believes that India's large pool of trained personnel can be of substantial utility to the people of Africa. And with a positive approach to economic and technological cooperation, India will not only win the confidence of the Africans, but make life more bearable for those members of Africa's Indian community who remain there.

Additional articles in this book examine Indian communities on the Indian Ocean island of Mauritius, the role of Asians in Kenya's independence movement, as well as Indian contributions to Africa's quest for equality, its need for technology and overall cooperative endeavors. The book concludes with a unique study comparing urban growth in India and Africa.

The reader will find a mixed bag in this collection of articles. The perspective, however, is a rare one and there is no ignoring the insights the book provides into contemporary Indian thinking about their country's role in an expanding and more demanding world.

LAWRENCE ZIRING

Western Michigan University

The European Parliament: The Three-Decade Search for a United Europe. By Paula Scalingi. (Westport, Conn.: Greenwood Press, 1980. Pp. x + ° 221. $18.95.)

The European Parliament has not attracted a great deal of scholarly attention until quite recently. It is hardly surprising that the subject has not been popular, since the European Parliament was for many years a nominated body with only a very modest, advisory role in European Community affairs—in fact, not a parliament at all. The change came in June 1979 when, for the first time, the Parliament was elected by direct, universal suffrage in the nine members states of the European Community. At the time, many viewed this as a development of some significance in the history of European integration. Paula Scalingi's informative book is in some ways a chronicle of how, after 30 years of effort and compromise, this milestone was reached.

As a chronicle, *The European Parliament* is very successful. It is meticulously researched and packed with information from many different sources on both sides of the Atlantic, many of them rarely found in English-language works. Unfortunately, however, the author does not go much beyond the historical approach and thus the potential for analysis, explanation and speculation is for the most part lost.

Scalingi suggests both in the subtitle of the book: "The Three-Decade Search for a United Europe" and in the preface that there is a correlation between the fortunes of the Parliament and moves in the direction of "political integration." If this is indeed the case, are we then to assume that the European Community is holding to the aims originally enunciated in the Treaty of Rome and that the Parliament, despite its lack of formal powers, has become an important building block in some form of political community? There is not much evidence to bear this out. In fact, the direct elections decision came at a time when many observers both inside and outside the Community were suggesting that things were going badly. Nor is there any solid evidence provided to support Scalingi's thesis that there is a "predictable cyclical pattern that has determined the course of European unification and the development of a real European Parliament" (p. x).

The whole question of the nature of a "real European Parliament" is a fascinating one that deserves thorough examination. The Parliament as it has existed until now fits no institutional category. It is widely recognized that it has greater significance than other international assemblies and less than the assemblies of truly federal systems of government. Can its limited powers and impact be explained by internal factors such as the absence of a constitutionally established government at the Community level, or is it necessary to look further for an explanation, perhaps to the decline of parliaments throughout Western Europe?

In many ways the European Parliament is an institution in search of a role. It has all the trappings of a political body and yet partisan battles are rarely fought out on the floor of the house. Indeed, distinct party groupings were present from the beginning yet one looks in vain for the development of the kinds of adversary relationships that exist between members when they sit in their national parliaments. Direct elections have only crystallized the party structure and 1979 even saw the emergence of transnational party platforms. All of this notwithstanding, the observer cannot help feeling that the European Parliament may now look more like a "real parliament," but that is as far as it goes.

By confining herself almost exclusively to a documentary treatment, the author of *The European Parliament* has lost a golden opportunity to provide the student of European integration and parliamentary process alike with much needed insights into some substantive theoretical problems.

GLENDA G. ROSENTHAL

Columbia University

To Augur Well: Early Warning Indicators in World Politics. Edited by J. David Singer and Michael D. Wallace. (Beverly Hills, Calif.: Sage, 1979. Pp. 308. $17.50, cloth; $8.95, paper.)

This book belongs to the growing class of edited volumes devoted to examining aspects of international politics from a quantitative perspective. Essentially, it offers a set of ideas about warning indicators: deciding which ones are important and how to measure them; what particular concepts they represent and the form describing their abstract-to-concrete linkage; and overall, how conceptually clarified, numerically specified variables permit the analyst to account for and predict the events and activities characterizing world affairs.

Nine papers and a composite introductory piece are brought together around two central topics: early warning indicators and contemporary forecast methodology. The volume includes "only original unpublished work that reported genuinely reproducible, policy-relevant findings, and reflected a high standard of data-generation and analysis" (p. 13). Following an unwritten rule, but a growing custom in works of this nature, the volume divides rather neatly into sections of empirical applications that use in turn, the systematic approach, a national attribute's framework, and analysis focused on national behaviors.

With respect to substantive concentration, no particular problem area dominates. A variety of issues is treated in these pages, among them: alliances, international and domestic conflict, nuclear proliferation, crisis perception, and foreign policy orientation. Obviously, these topics are important ones befitting international politics, but it is not clear to the reader just how the chapters are intended to produce a composite, comprehensive, or assembled picture that captures a fresh look at the field. This is not the avowed purpose of the collection, however, and the editors refrain from suggesting any plan of substantive cohesiveness across topical domains addressed here.

The book is accurate, I think, in its *diagnosis* of intellectual, theoretic, and policy-making problems that face those charged with anticipating and comprehending world events. Michael Wallace's lead essay, "Early Warning Indicators from the Correlates of War," states:

> The past two decades dramatically exposed the inability of traditional "experts" in the world politics field to predict even the rough outlines of the changes taking place in the global system. The list of their failures is legion: the outcome of the war in Indochina, the renewed strategic arms race that threatens one more, the fall of a large fraction of Third World civilian regimes to military forces, skyrocketing energy prices, and—as a partial corollary of this last—world-wide economic "stagflation" with its attendant monetary and trade rivalries. All of these (and more) either "couldn't happen" or were largely unforeseen by the keepers of the temple of conventional wisdom. Indeed, if our political and economic soothsayers were baseball players, their latter-day batting average would see most of them instantly sent down to the minors.
>
> At first blush, one might suppose that these failures would have a silver lining for peace researchers and other quantitative social scientists. Since armchair experts have been so often discredited, the way is surely open for those who advocate the application of scientific rigor to the tasks of forecasting and planning. But this has not taken place; rather the reverse has occurred. In recent years, the apparent resistance of policy makers to new intellectual approaches has, if anything, grown. This seeming paradox is, I believe, largely explained by reference to three factors, two relatively transitory phenomena, and one underlying, existential dilemma. These are, in order: (a) the rather small number of findings in the world politics which can readily and safely be used by either policy makers or counter-elites; (b) the "overselling" of the predictive power of social science that has occurred in recent years; and (c) inherent limitations on the impact *any* scientific knowledge can have on policy-making (pp. 17-18).

With some hesitation, it might be said that the articles, in sum, present a *prognostic* litany of these problem factors noted by Wallace. This is accomplished by showing corrective pathways to circumvent a specific obstacle (e.g., how to use research findings appropriately in a policy context); or at the very least, the papers provide commentary to help us understand why the current design and management of quantitative aids in public policy analysis is non-optimal with respect to information on early warning signs.

The papers are built around several major data collections extant in international relations and contain, as well, some infrequently used analytic techniques for sifting through and finding patterns in vast amounts of information. (Two of the nine have methodological appendices, one on the adaptive systems algorithm and procedures, and another on the Partial Least Squares model.) Authors and papers include: Wallace, "Early Warning Indicators from the Correlates of War Project"; Siverson and King, "Alliances and the Expansion of War"; Dewitt, "Breakpoints and Interstate Conflict"; Wilkenfeld, Hopple and Rossa, "Socio Political Indicators of Conflict"; Gurr and Lichbach, "Forecasting Domestic Political Conflict"; Meyer and Brewer, "Monitoring Nuclear Proliferation"; Sullivan, "Foreign Policy Articulations and U.S. Conflict Behavior"; Phillips and Rimkunas, "A Cross-Agency Comparison of U.S. Crisis Perception"; and Chan, Kringen and Bobrow, "A Chinese View of the International System."

How does one evaluate this "book" when the topics and techniques cover such broad ground? I have chosen only two issues for appraisal in light of Wallace's characterization: (1) Are there any significant generalizations or theoretic ideas to provide insight on the early warning question? Yes, indeed. Each piece expresses in its title a hint of the conclusions and general points to come. There are specific results too numerous to be given here. Among the noteworthy ideas with a touch of novelty or profundity are these: Dewitt's notion in "Breakpoints and Interstate Conflict" (more on the novel side) of a *breakpoint*, which he defines as an indication of fundamental change in the dynamics of international relations. His analysis of retrospective forecasting using macro-level variables to track major state activity culminating in World War I appears as a useful, scholarly exercise. Just how breakpoint possibilities could be monitored into the future, however, is unclear. Meyer's and Brewer's work in "Monitoring Nuclear Proliferation" is profound and clever. They lay out existing procedures for monitoring nuclear weapons proliferations, point to the inherent limitations to them, and then present a conceptualization for warning or assurance signals using technological capability, motivational profiles, and technological process monitors of nations. The scheme is diagrammed with useful figures and accompanying verbal descriptions. It is the best piece of the lot partly because the early warning conceptualizations could be applied to a variety of substantive areas beyond the *n*th country problem.

(2) What should policy makers expect from the analyses found here? Overall, the answer is that quantitative, computerized devices called "models" are highly intuitive representations of

the world, and are rightly approached as analytic perspectives that may or may not capture important features of a given problem. The aim of quantitative assessment is to set a framework to enhance and extend judgment by bringing together scientific objectivity and subjective interpretation so that a decision maker can understand explicitly the basis for action and attendant underlying perceptions of the situation. Each of these articles in its own way, provides support for a limited view of the impact of the discoveries of social science on policy choices. Numerous suggestions are made regarding monitoring impending, undesirable developments on the world scene. No method is foolproof or final. In short, there is no single, best way to track and watch the omens that may befall us, but some good ideas have been brought to light.

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Power Politics and Southeast Asia. By Lalita Prasad Singh. (Atlantic Highlands, N.J.: Humanities Press, 1979. Pp. xi + 208. $12.50.)

A special focus on the international crises of the moment is a prominent and even necessary characteristic of the foreign policy operations of all the countries of the world. The United States, however, seems to be particularly prone to exaggerate the immediate issues, frequently almost to the exclusion of all other aspects of international affairs. Thus, as this review is being written in early June 1980, the core issues (although fading) are U.S. hostages in Iran, Russians in Afghanistan, and the Olympic boycott. As for Southeast Asia, if we discount the recent drama over the "boat-people" refugees from Indochina, we might well conclude from our popular as well as more academic literature that the fall of Saigon in 1975 was for Vietnam as well as Southeast Asia in general a fall not only from grace but also from earth. In this context, Lalita Prasad Singh's overview of the foreign affairs of the Southeast Asian states should be an important book.

Part 1 (The Great Powers and Indochina) includes separate chapters on Vietnam, Laos, Cambodia, and the ICC (the truce supervisory commission established by the Geneva Accords of 1954). Part 2 (Regional Power Politics) focuses on the interaction between and among Indonesia, Malaysia, and the Philippines, while part 3 (New Trends and Developments) covers Sino-Soviet rivalry in Indochina, developments in Thai foreign policy and in that of Malaysia, and the prospects for regional neutralization and economic cooperation and organization. The concluding chapter raises the issue of "two Southeast Asias"—Indochina on the one hand and the Association of Southeast Asian Nations on the other—and their relations with outside powers. Pertinent international documents are included in seven appendices.

The central topic is described by the author as "Great Power rivalry [i.e., between the United States and the Soviet Union] for a sphere of influence in Southeast Asia and the response of the regional states to external intervention. . ." (p. x). In fact, however, the study is broader than this in that substantial attention is devoted both to the roles of India and China in the region and to the increasingly important bilateral and multilateral relations within the region itself.

Although this is a timely book on a topic of continuing importance, it is out of balance in a number of instances. For example, the reader receives little information about U.S. purposes in Vietnam or why failure occurred. On the other hand, the study offers more than enough detail about Sukarno's "Crush Malaysia" campaign and other aspects of Indonesia's foreign policy in the 1960s prior to the fall of Sukarno.

In a more general sense the dominant focus of the book overall is the 1950s and, particularly, the 1960s with rather skimpy coverage of more recent events. This criticism notwithstanding, Singh's attempt to take a long-range view, a view which is frequently not taken by the general public, the practitioner, or even the academician, is most helpful in placing the present into a meaningful context. Moreover, the discussions of particular aspects of this historical context are generally succinct and quite good. Examples of these are the author's treatment of the Soviet perception of Indochina in the immediate post-World War II period, the context and explanations for Singapore's inclusion and then later expulsion from Malaysia, the reasons behind Thailand's Northeast "problem," and the issues involved in the Sino-Soviet rivalry in Indochina.

Although this study offers little new and compelling for the technician, it nevertheless is a useful and competent wide-ranging treatment of the major issues involved in the international affairs of the Southeast Asian states. Because of this, it should receive substantial attention.

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The Joint Organizations of Canada and the United States. By William R. Willoughby. (Buffalo, N.Y.: University of Toronto Press, 1979. Pp. xi + 289. $25.00.)

In his research extending over more than three decades, William R. Willoughby has established himself as an astute and prolific scholar of Canadian-American relations and he continues his valuable contributions to the literature in this book. Employing a rich variety of sources including government documents, archival materials, and personal interviews and communications, Willoughby has constructed a record of the unique practice of Canada and the United States to use joint organizations to deal with myriad policy issues transcending the international boundary.

Willoughby's account is a sharp descriptive overview of the evolution, responsibilities, problems, successes, and failures of these joint organizations. He does not hesitate to offer praise or criticism where appropriate, both to the organizations and to the governments. Although other scholars have examined these joint organizations, Willoughby has effectively considered the important lessons and activities of each one and has probed the relationship of the organizations to the existing condition of general Canadian-American relations.

He emphasizes that although the joint organizations have made important contributions to the facilitation of bi-national relations, they are a supportive method of communication and problem solving between the two governments on the issue, but not the sole method. Some of the organizations, especially the more technically oriented ones such as the International Joint Commission or the various fishery commissions, are very active, but others, such as the Ministerial Committee on Trade and Economics, have fallen into disuse. Recently both governments have on occasion adopted a policy of using ad hoc committees or discussions to deal with certain issues that might properly be referred to one of the joint organizations. Willoughby asserts that this development does not represent a vote of no confidence in the joint organizations but rather a recognition that certain pressing issues (especially those involving matters of high policy) may be more effectively handled by direct and focused diplomacy.

It is clear from Willoughby's account that the joint organizations have always functioned within an existing political environment. An aspect of this environment is the conviction by the leaders of both states that the bilateral issues should be resolved as harmoniously as possible and to the mutual benefit and accommodation of both states. Another is the awareness that among the two states there are substantial differences in economic capacity, military power, and international responsibilities that inevitably have an impact on their bilateral relations. Because of this disparity in power and capacity, Canadians have always been alert to the danger of being consumed or overwhelmed by a well-meaning but likely insensitive neighbor. Consequently, Canadian participation in joint organizations has frequently involved a delicate balancing act of promoting mutual cooperation and accommodation and at the same time seeking to protect Canadian sovereignty, identity and independence. The controversy over Canadian participation in NORAD is an example.

It is equally clear that the use of the joint organizations has not served to resolve all of the controversy or differences surrounding a particular issue. Indeed, agreement within the joint organizations has occasionally been followed by sharp disagreement. In addition, the Canadians have sometimes expressed their unhappiness with unilateral American action in situations wherein bilateral consultations through joint organizations would appear to be warranted and expected.

Although specialists in Canadian-American relations will be familiar with the various joint organizations that Willoughby studies, they will be rewarded by his thorough research and clear assessments. Non-specialists who are interested in the types of institutional arrangements that states can create to deal with mutual problems should place Willloughby's book at the top of their reading list.

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Compliance and Public Authority: A Theory with International Applications. By Oran R. Young. (Baltimore: Johns Hopkins University Press, 1979. Pp. x + 172. $12.95.)

This book is important because it tackles one of the most fundamental questions confronting policy makers in international politics: can compliance with international agreements be obtained in decentralized social systems with no central authority? Oran Young questions the related arguments that the major problem with the international system is the absence of centralized institutions of government and that international law is not really law because of the absence of formalized enforcement mechanisms. Based on an analysis of two case studies, the Partial Test Ban Treaty (PTB) of 1963, and the International North Pacific Fisheries Convention (INPFC) of 1953, Young's conclusion is clear: despite much past thinking, compliance can be gained in decentralized systems. Compliance with the PTB shows an "impressive record" (p. 62) and has been "excellent to date" (p. 66), and for the INPFC, compliance with the prescriptions "is relatively high on the part of all Contracting Parties" (p. 87).

Although Young demonstrates that there has been compliance in both cases, the strength of his conclusion is facilitated only by his overlooking several problems. First, Young overemphasizes the importance of any manifestation of compliance behavior. The lack of centralized institutions, however, has rarely been equated with total anarchy; few have contended that compliance with either written or tacit agreements is totally absent from the international system.

In addition, although Young's two case studies provide convincing illustrations of compliance among signatories, they also provide ample instances of noncompliance. We find, for instance, that the "general objectives" of the preamble of the PTB have not been fulfilled, that there have been large-scale increases in underground testing (p. 59), and that one authority has concluded that the post-PTB system is "what many believe is a nuclear-weapons development effort that is indistinguishable in scope—if it is not even larger—from the one that had been under way before the treaty" (p. 59). Among the nations that did not sign the treaty, France and China alone conducted 53 nuclear explosions during the 1963-1973 period, 45 of them in the atmosphere. As of 1974, about half of the 1,012 nuclear explosions since 1945 were carried out after the PTB (p. 59).

In the case of the INPFC, too, we find conflicting evidence of compliance. Within the strict confines of the convention itself, there are many instances of compliance among the three signatories (the United States, Canada, and Japan), who were until recently the only nations with sufficient mutual fishing interests to warrant a convention and compliance with it. However, the recent growth of high seas fishing operations in the North Pacific by noncontracting parties (the USSR, Korea, and Poland) "has produced serious difficulty for the INPFC regime. . ." (p. 69). In short, we do not know whether to call the glass half empty or half full. Young ultimately opts for the more optimistic view, but the evidence seems equally consistent with the opposite conclusion.

Indeed, on the last page of his case study, Young reveals that the INPFC regime is actually "collapsing under the weight of several factors" (p. 93), including the expanding operations of noncontracting states, and the extension of coastal state jurisdiction over marine fisheries. Although the convention itself does not preclude the inclusion of these other states, according to Young, "it has not proven politically feasible to move in this direction" (p. 69). He does not speculate on why this is so, but one guess is that the noncontracting parties have not joined because it is not in their interest to join. In other words, compliance may be related simply to the individual interests of the concerned parties—an unfortunate defect in the convention, but one that nevertheless illustrates the special character of the international system.

Thus, member nations have strong incentives to avoid violating the provisions of the PTB, because compliance does not prevent them fro pursuing most of their key objectives. Violations that do occur, moreover, are discernible even without inspection. Although compliance is costly to INPFC signatories, nonetheless, Young notes, with respect to the clauses on conservation, that "these parties would have compelling reasons to formulate and carry out similar programs of conservation even if the INPFC did not exist" (p. 75).

This book paints a correctly complex picture of compliance, and although emphasis here has been on the two case studies, the book is not limited to these cases. The two other sections consist of lengthy elaborations of definitions, concepts, assumptions about behavior, and scenarios describing potential compliance behavior under various conditions. However, this conceptual apparatus seems to be virtually divorced from the case studies: no reference to either case can be found outside of their respective chapters. Moreover, while thorough and detailed, it is, contrary to the subtitle, not so much a theory in anything approaching the formal sense, but rather the conceptual apparatus necessary to begin working on a theory of compliance. But any work on complliance, especially related to the international system, must not view any evidence of compliant behavior as a major breakthrough, and would ideally try to establish some criteria of the importance of the compliance behavior that does occur. Young's work is rich and complex, and goes much beyond the mostly normative and simplistic analyses of compliance in international relations that have been produced in the past.

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